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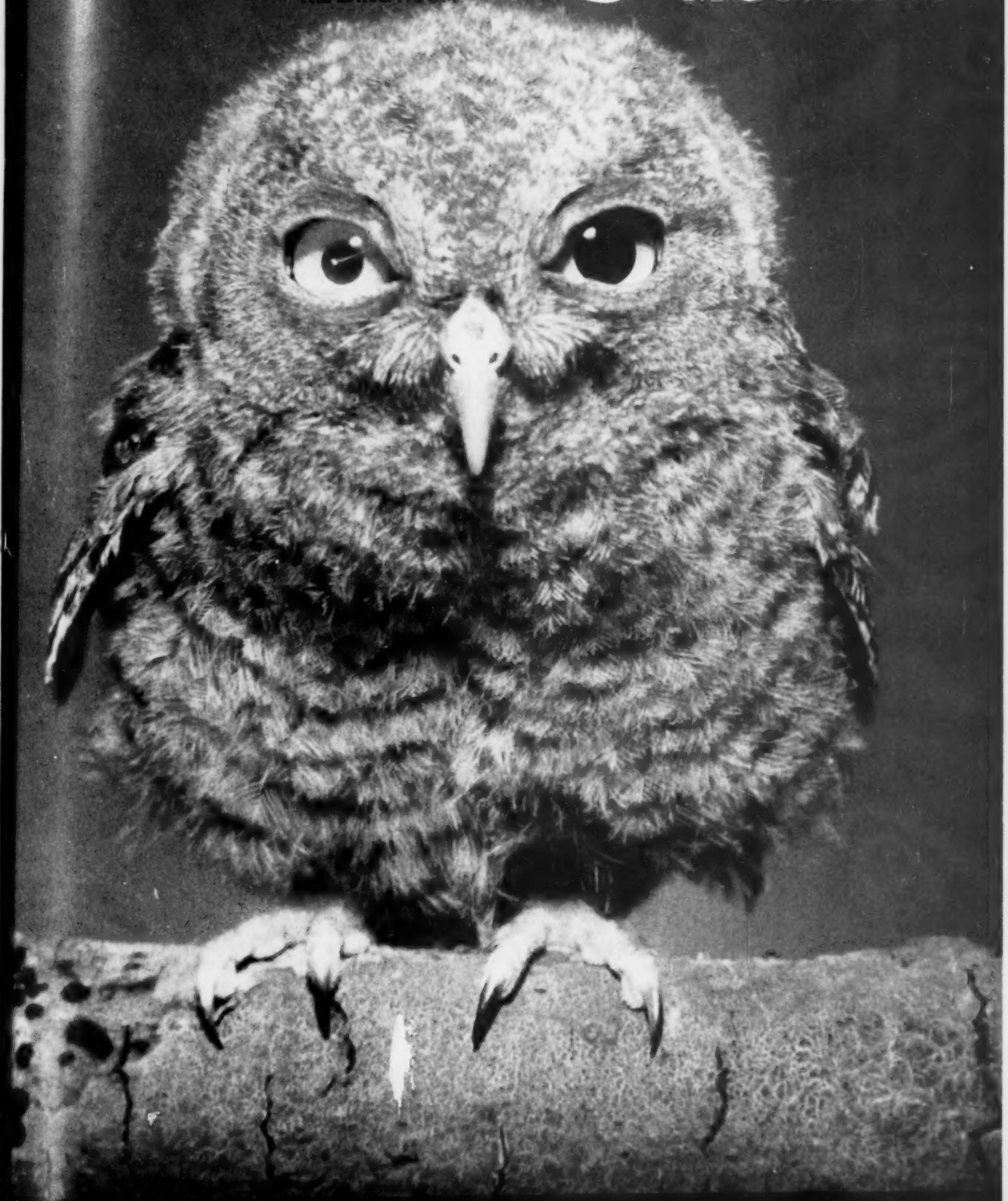

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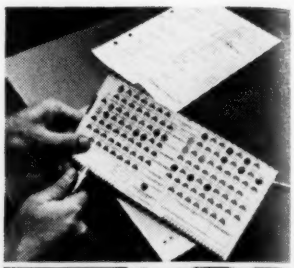

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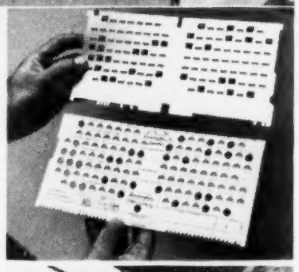

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THE SCIENTIFIC MONTHLY

VOL. 80 MARCH 1955 NO. 3

The Oldest Tetrapods and Their Forerunners . . . . . *Erik Jarvik* 141

Sherlock Holmes as an Anthropologist . . . . . *Wilton M. Krogman* 155

Latent-Image Formation and Chemical Sensitization . . . . . *W. F. Berg* 163

Identification and Guidance of Gifted Children . . . . . *Antonia B. Morgan* 171

New Ideas in Chemistry . . . . . *John Lennard-Jones* 175

The Language of Science . . . . . *Charles E. Whitmore* 185

Association Affairs: *Gilbert F. White and Peter C. Duisberg, Dael Wolfle, Raymond L. Taylor* 192

Letters from *Jay A. Young, Adolf Grünbaum, Richard T. Vineski, Alexander Gode, J. M. Martinez, Jerome Richfield, Bayard* and *Evelyn McConnaughey* 200

Book Reviews of *The Inside Story: Psychiatry and Everyday Life; Fundamental Principles of Bacteriology; Laboratory Manual on Fundamental Principles of Bacteriology; Introduction to Mathematical Statistics; General Chemistry; Aspects of Culture and Personality; Science and Civilization in China; Atomic Energy: A Survey; Human Development; Geometry of René Descartes; Present State of Physics; Entomology (Medical & Veterinary); Astrophysics: Nuclear Transformations, Stellar Interiors, and Nebulae; Elementary Statistics; Medicine and Science*; Books Reviewed in *Science*; and New Books 203

Cover: Screech Owl with Left Pupil Larger than Right

[*Courtesy A. M. Winchester, Stetson University, DeLand, Florida, see page 162*]

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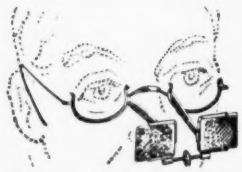

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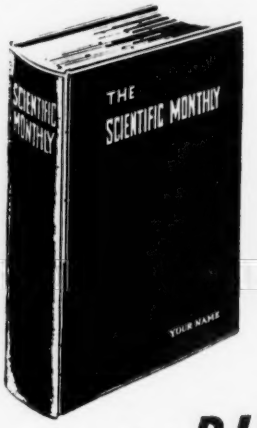

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Science and Technology

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Hand Snow Plow

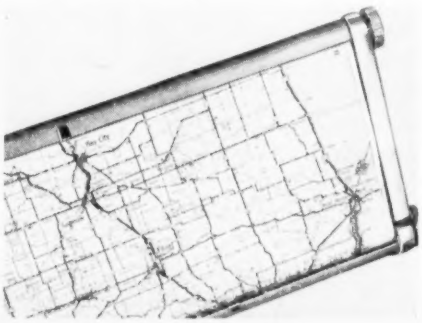
A new all-steel snow plow has a 12- by 30-in. blade that can be adjusted to push snow to the left, right, or straight ahead by means of springs controlled by wing nuts. The plow rolls on 6-in. rubber tires. (Leisure Industries, Dept. 506-SM, 96-09 Metropolitan Ave., Forest Hills 75, N.Y.)

Rust Converter

FerRoSeal, a product developed by the Walterisation Co., Ltd., Croyden, England, is a liquid that chemically converts rust into a protective iron phosphate coating, which is said to be a better paint base than clean steel. In addition, the product works in harmony with other coatings such as coal-tar products, zinc-base materials, and neoprene. (Municipal Steel Corp., Chemical Div., Dept. SM, 1225 Broadway, New York 1.)

Map Holder-Sun Visor

The Map-Visor is a combination sun visor and road map holder designed to replace standard sun visors in automobiles. The unit can be mounted over either the left- or right-hand seat and can be detached easily for changing maps or for close scrutiny. The particular area desired can be selected by turning the rollers that hold the map at the top and bottom of the visor. Sliding pointers on the rollers make it possible to mark the end points of the route being traveled. Two models, one with illumination and the other without, are available. (Rockford Engineered Products Co., Dept. SM, 2324 23rd Ave., Rockford, Ill.)



Wire Stripper

Fabricated of hardened steel, a new hand wire stripper has interchangeable matched blades for stripping insulation without damaging the wire. After the wire has been stripped, the jaws of the stripper can be released by a single finger-tip touch on the lower handle. The jaws are then locked open automatically and are ready for the insertion of the next wire. The stripper is available in three models. Replacement blades, which can be installed after removal of two screws, are also available. (Crown Industrial Products Co., Dept. SM, 1335 Amsterdam St., Woodstock, Ill.)

Insulation Test Set

A new model Hypot insulation test set rated at 0 to 5000 v a-c has continuously variable output voltage, a current-limiting high reactance transformer, neon lamps to indicate corona discharge in the test circuit and excessive current, and a kilovoltmeter accurate to ±3 percent connected across the output. (Associated Research, Inc., Dept. SM, 3758 W. Belmont Ave., Chicago 18, Ill.)

Coil Winder

Toroid coils having a finished inside diameter of ⅛ in. can be wound with a new MidJet toroid winder. The winder is 9½ in. long. Wire in sizes from No. 30 to 44 can be wound on closed magnetic cores of wound tape, stacked ring punchings, or molded powder. Winding speeds range from 400 to 600 turns/min, and the turns are laid radially under adjustable tension. Polished guides prevent abrasion of insulation. Manual rotary core feed permits clockwise or counterclockwise progress of the winding; both layer winding and bank winding are possible. A register-type counter records the wire loaded on the shuttle and counts the turns. (Electro Devices Co., Inc., Dept. SM, 463 Commercial St., Boston, Mass.)

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A new siphon 5 ft long has a bulb-shaped hand pump in the middle that permits starting the siphon without spilling. The bulb and tubing are made of synthetic rubber and the check valves of plastic. (General Scientific Equipment Co., Dept. SM, 2700 W. Huntingdon St., Philadelphia 32, Pa.)

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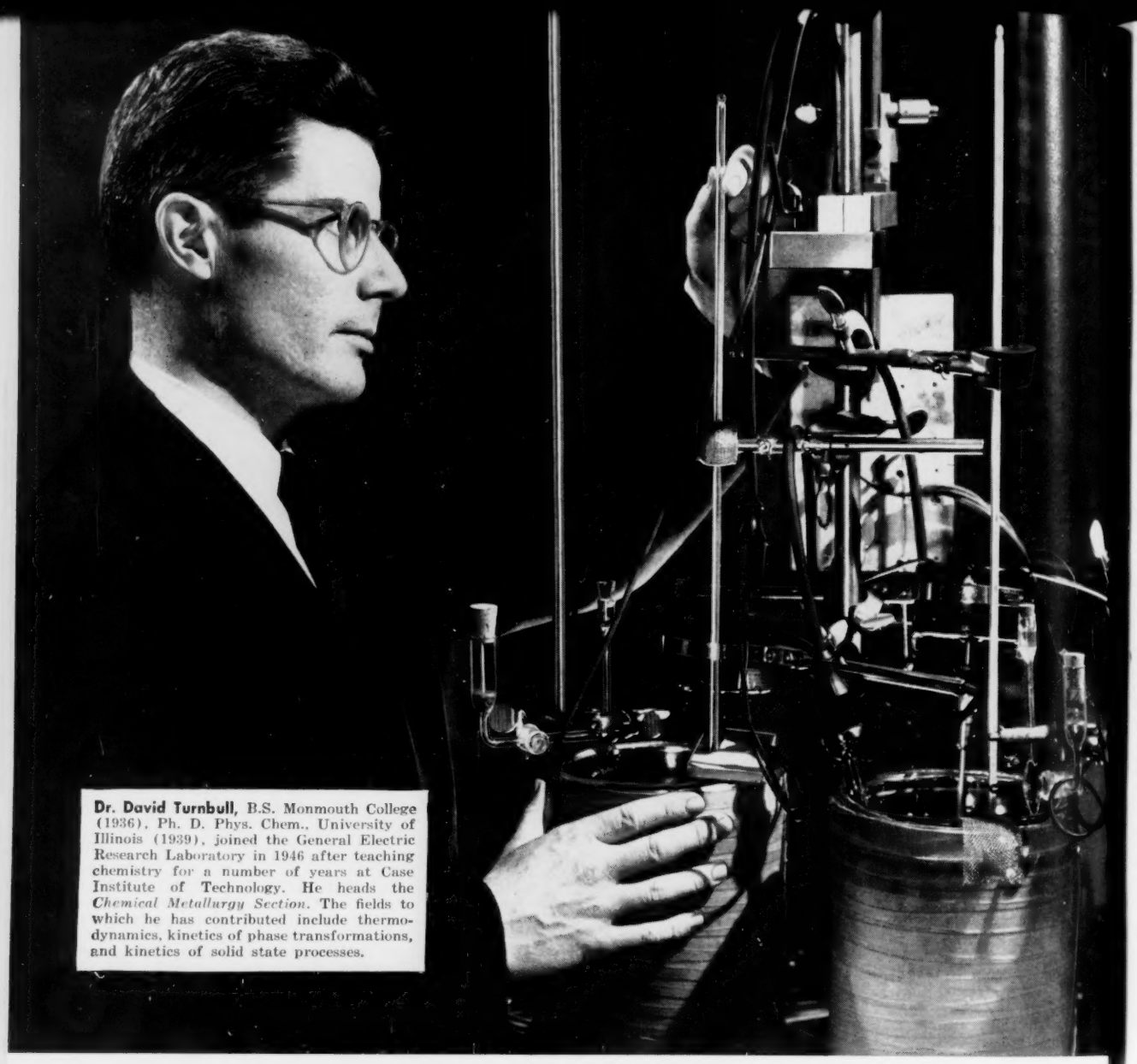

Dr. David Turnbull, B.S. Monmouth College (1936), Ph. D. Phys. Chem., University of Illinois (1939), joined the General Electric Research Laboratory in 1946 after teaching chemistry for a number of years at Case Institute of Technology. He heads the *Chemical Metallurgy Section*. The fields to which he has contributed include thermodynamics, kinetics of phase transformations, and kinetics of solid state processes.

How atoms behave in alloys

Dr. David Turnbull of the General Electric Research Laboratory demonstrates importance of crystal defects in structure changes

New understanding of the mechanisms responsible for the formation of alloy structures has been achieved by Dr. David Turnbull and his colleagues in General Electric's *Chemical Metallurgy Section.*

They discovered that crystal imperfections and foreign particles play a decisive role in controlling structure development. For example, precipitation in age-hardening alloys often takes place millions of times faster than can be explained by known rates of diffusion in single crystals. Dr. Turnbull showed that this rapid precipitation is due to the "short circuiting" of diffusion by certain crystal defects.

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The Oldest Tetrapods and Their Forerunners

ERIK JARVIK

Dr. Jarvik is curator in the paleozoological department of the Swedish Museum of Natural History in Stockholm and lecturer in vertebrate paleontology at the University of Stockholm. His special interests are the Devonian fishes, particularly the crossopterygians, and the early tetrapods. Dr. Jarvik was in charge of Devonian fossil collecting during six Danish expeditions to East Greenland under the leadership of Lauge Koch between 1932 and 1954; he was also a member of the English-Norwegian-Swedish paleontologic expedition to Spitsbergen in 1939. This article is based on an address given at the International Zoological Congress, Copenhagen, 1953.

SINCE the days of Darwin the question of the origin of the tetrapods has attracted great attention among anatomists, embryologists, paleontologists, and other students interested in evolutionary biology; but even though it has long been known that the tetrapods are descendants of fishes, it has been difficult to establish from which group or groups of fishes they derive their origin. Three groups have played a predominant role in the discussion: (i) the lungfishes (Dipnoi), known as fossils since the Devonian and today represented by the Australian lungfish (*Neoceratodus*) and others; (ii) the bichirs (Brachiopterygii), including only the two living genera *Polypterus* and *Calamoichthys*; and (iii) the lobefins (Crossopterygii; including rhipidistids and coelacanthids), abundant particularly during the Devonian but thought to be extinct since the Cretaceous until, in 1938, a living coelacanthid (*Latimeria*) was caught in the sea off East London on the southeast coast of the Union of South Africa.

Earlier the interest was concentrated on the living lungfishes and brachiopterygians, but the efforts made to prove a close relationship between either of these groups and the tetrapods gave no convincing results. Progress in vertebrate paleontology slowly brought the fossil fishes into the foreground, and during the last few decades most students have claimed that the immediate forerunners of the tetrapods are to be found among the crossopterygians and dipnoans of the Devonian period.

Some investigators (*1–6*) are of the opinion that all tetrapods have a common origin among the rhipidistid crossopterygians, whereas others maintain that the Tetrapoda are diphyletic and that the urodeles (salamanders) are most closely akin to (*7*), or descendants of (*8*), early dipnoans and that the stegocephalians, the anurans (frogs and toads), and all Amniota (reptiles, birds, mammals) originate from rhipidistids.

The dipnoans, which earlier were classified separately, are now generally united with the crossop-

terygians in a main group of teleostome fishes, the Choanichthyes, distinguished by the presence of a true internal nostril or choana. It has been argued (*3*, *7*) that the Choanichthyes differ widely from the other main group of teleostome fishes, the Actinopterygii (paleoniscids, sturgeons, teleosts, and so forth). The latter group is now generally made to include the brachiopterygians, which were once thought to be crossopterygians. Most writers assume that both the dipnoans and the coelacanthids are specialized derivatives of the rhipidistids, but it has also been suggested (*1*) that the dipnoans are more closely related to the armored sharks or placoderms, and one writer (*9*) classifies the dipnoans among the Amphibia and maintains that all tetrapods are descendants of the chimaeroids (holocephalians).

The thorough studies (*10–15*) made during the last two decades of the Devonian crossopterygians and dipnoans and of the oldest tetrapods, the Ichthyostegalia, have not wholly confirmed any of these various opinions, but have shed new light on the ancestry of the tetrapods and the interrelationships of the principal groups of fishes (Dipnoi, Crossopterygii, Brachiopterygii and Actinopterygii) generally included in the Teleostomi (Fig. 1).

Methods of investigation. In the studies of the fossil material described in this article, the elaborate technique that was introduced by E. Stensiö in his classical monograph on the cephalaspids from Spitsbergen (*16*) has been used. This technique includes serial sectioning by grinding and the construction of large-scale wax models from the serial drawings; various special methods for freeing the skeletal parts of the fossils from the rock so that they can be studied just as minutely as recent vertebrates; the examination of the fossils under high-powered binoculars in strong illumination and with the use of xylene, oil of aniseed or other refracting fluids; photographing, often in fluids, and careful retouching of structural details; and so forth.

The ancient vertebrates of the Silurian and

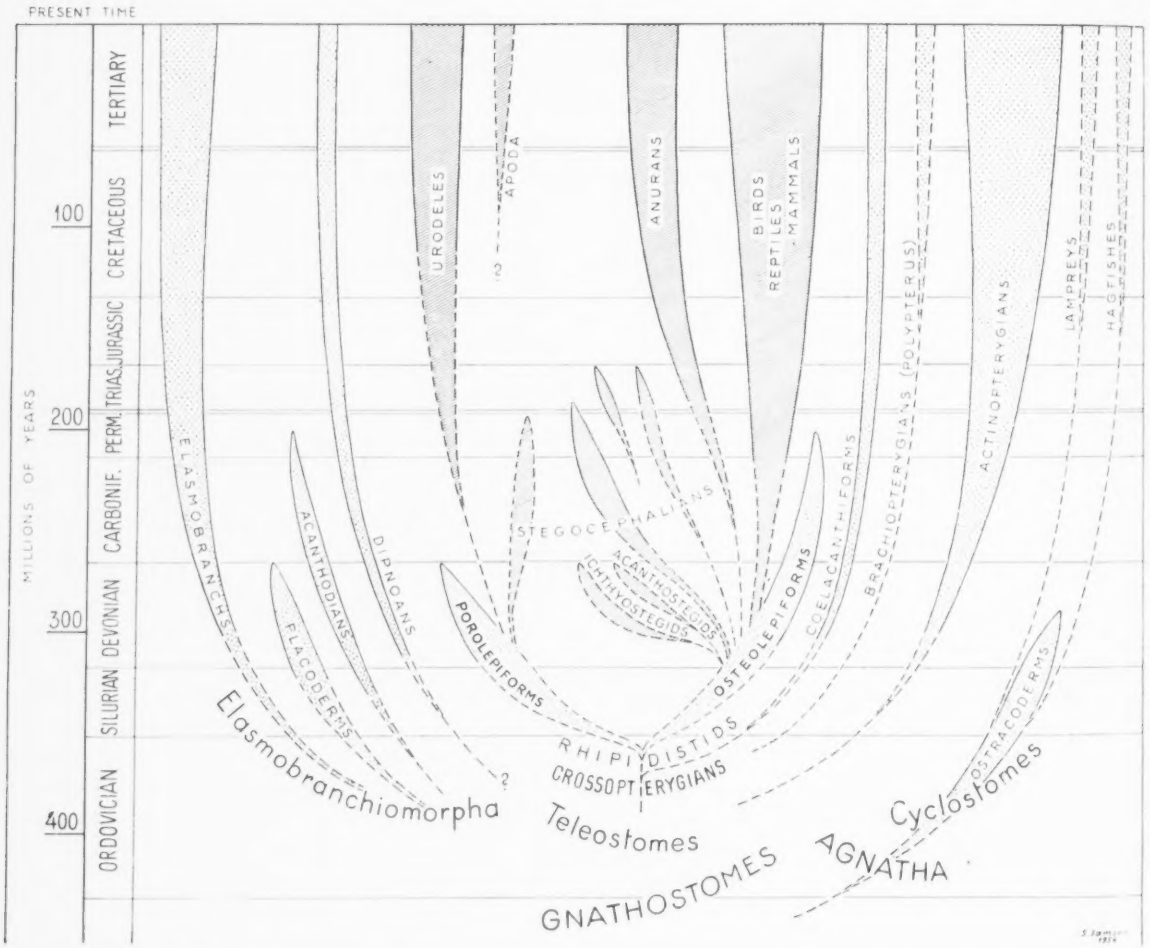

Fig. 1. The interrelationships among the main groups of vertebrates. Tetrapods hatched; cyclostomes and fishes stippled.

Devonian have a quality in common of great importance for the paleozoologist: their skeleton is generally well ossified and on the whole more complete than in later vertebrates where it often may be represented by tissues devoid of lime impregnation. This of course has strongly favored the preservation of the early vertebrates. Besides—a matter frequently overlooked by students familiar only with material of recent vertebrates—the high degree of ossification in the early vertebrates means that the cavities (for the brain, the ear, and other organs) are comparatively narrow, reflecting fairly well the external shape of the organs they housed, and that the spaces occupied by muscles, glands and other soft structures are more clearly defined than in advanced forms. The bone tissue also encloses long canals, displaying well the course and mode of branching of the nerves and vessels. In addition, the faces of the bones frequently exhibit distinct grooves and other markings of anatomical interest. When applying the modern paleozoological technique it is therefore often possible to obtain an intimate and safe knowledge of the skeleton with its cavities, passages for nerves and vessels, muscular attachments, and other features.

Considering the wealth of structural detail that can be made out from the skeleton in fossil forms by the aid of modern methods the paleozoologist must be thoroughly initiated in the anatomy and embryology of recent vertebrates. Because of the fact that the anatomy and embryology of the recent vertebrates are still imperfectly known, the interpretation of the fossil structures necessarily must be a laborious and trying task. This is the more so since in most cases the paleozoologist must collect relevant data from a great many descriptions, often conflicting, and mostly incomplete, above all with regard to the topographic relations between the soft structures and the skeleton. Frequently the paleozoologist must even make his own dissections of recent material.

The much wider knowledge of the internal anatomy of the early vertebrates gained during the last few decades has naturally rendered it possible much better than before both to determine the interrelationships of the various groups and to form an opinion of the phyletic evolution of the vertebrates in general. Thus, for instance, Stensiö (*16*) was able to prove definitely by his anatomical investigations of the cephalaspids that the armored and dorso-ventrally depressed ostracoderms, the geologically oldest vertebrates known so far, are primarily jawless (agnathous) vertebrates related to the recent eel like cyclostomes (lampreys and hagfishes), which are devoid of armor and even of bone tissue. Moreover, Stensiö could establish that,

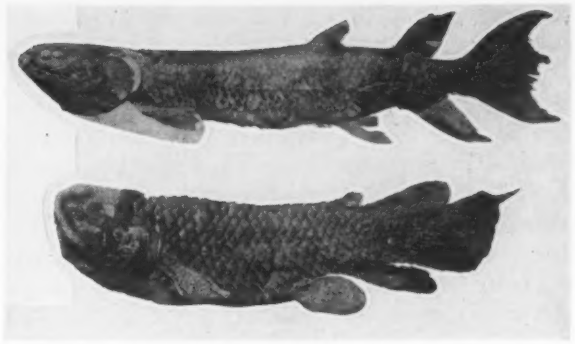

Fig. 2. Two rhipidistid crossopterygians from the lowermost Upper Devonian, Scaumenac Bay, Canada. Above, an osteolepiform, *Eusthenopteron foordi*, length about 53 cm. [Specimen in the paleozoological department of the Swedish Museum of Natural History, Stockholm]. Below a porolepiform, *Holoptychius quebecensis*, length about 46 cm. [Specimen belongs to the Dartmouth College Museum, Hanover N.H.]

with regard to anatomy, the cephalaspids agree fundamentally with the lampreys, especially with their larvae (ammocoetes), whereas the hagfishes are different in many respects and doubtless belong to another lineage. During the phyletic development of the lampreys from cephalaspidlike ancestors the internal anatomy has on the whole remained much more unchanged than the external appearance of the animals. The changes in the latter respect are partly the result of the regressive development of the skeleton, which caused first a disintegration and then a total loss of the bony skeleton.

What has just been said suffices to illustrate the impossibility of making out the true interrelationships among different groups of vertebrates solely on the basis of superficial external resemblances or general specialized characters. For this purpose profound anatomical investigations are always indispensable.

The origin of the tetrapods. The studies of the origin of the tetrapods carried out in Stockholm (*10*, *12–15*) are based on a large and in many respects unique collection of Devonian crossopterygians and dipnoans and the only existing material of the oldest tetrapods, the ichthyostegalians, from the Upper Devonian of East Greenland.

Even when they first appeared in the Lower and Middle Devonian, the crossopterygian fishes (Fig. 2) were represented by three distinct groups: the Porolepiformes, the Osteolepiformes and the Coelacanthiformes (*10*). These groups have several characteristics in common, such as, for instance, a subdivision of the braincase into anterior and posterior divisions (ethmosphenoid and otico-occipital, Fig. 3) and a corresponding transverse subdivision of the dermal cranial roof into an anterior

frontoethmoidal shield and a posterior parietal shield (Fig. 4).

The three groups of crossopterygians, the porolepiforms, the osteolepiforms and the coelacanthiforms, differ from each other particularly in the internal anatomy but also to some extent in the development of the dermal bones, the sensory canal system, and other external features (*10, 11, 13, 17*). In the anterior part of the head, which is the only region of the head so far known in detail in each of the three groups the differences are in fact so great (Figs. 5, 6) that if only this region had been known, they would hardly have been placed in a common systematic unit.

In fishes in general the nasal sac has two principal external openings, the anterior (incurrent) and the posterior (excurrent) external nostrils. In the crossopterygians the posterior external nostril was connected with the nasal sac by a posterior nasal tube (Fig. 5). This tube is homologous with the nasolachrymal duct in tetrapods. It is of special interest, then, that the nasal sac in the osteolepiforms and porolepiforms had a third major opening, an internal nasal opening or choana, homologous to that in tetrapods. The choanal tube connecting this opening with the nasal sac passed through a well defined fenestra endochoanalis (Fig. 5) in the floor of the nasal cavity and a corresponding fenestra exochoanalis bounded by the dermal bones of the palate. In the fossil coelacanthiforms, on the other hand, there is no opening for the choanal tube, either in the endoskeletal walls of the nasal cavity or in the exoskeleton, and, as could be definitively established, for example by the serial grinding method, the choana was absent in the early coelacanthiforms just as in the living *Latimeria*. Since there is nothing to indicate that the choana was lost its absence must be regarded as primary.

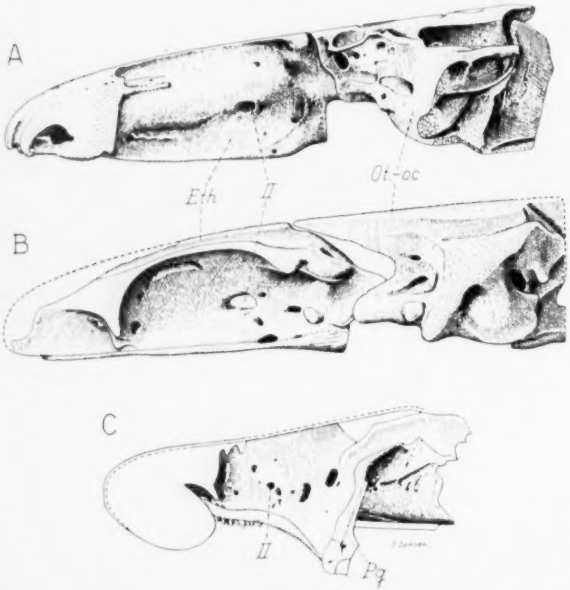

Fig. 3. The lateral aspects of the braincase and (in *C* only) the palatoquadrate of, *A*, an osteolepiform crossopterygian (*Eusthenopteron foordi*), *B*, a coelacanthiform crossopterygian (*Nesides schmidti*), and *C*, a dipnoan (*Chirodipterus wildungensis*)—all early Upper Devonian in age. *Eth*, anterior ossification (ethmosphenoid) of braincase; *Ot.-oc*, posterior ossification (otico-occipital) of braincase; *Pq*, palatoquadrate; *II*, opening for nervus opticus. [*A*, *B*, after grinding series and wax models in the paleozoological department of the Swedish Museum of Natural History, Stockholm; from Jarvik 1954 (*13*). *C*, from Säve-Söderbergh 1952 (*15*)]

As may be seen from what has now been said the three groups of crossopterygians have become specialized in quite different directions. Since the numerous structural features characteristic of each of the three groups were fully developed in their oldest members, it is quite obvious that these specializations must have arisen very early in the phylogeny of the crossopterygians and certainly before the Devonian. The current view that the coelacanthiforms are descendants of osteolepiforms (or porolepiforms) cannot possibly be true and we cannot expect to find any true intermediates ("protocoelacanthids") between the osteolepiforms and the coelacanthiforms high up in the Devonian. In fact, hardly a single one of the many structural features characteristic of the coelacanthiforms has been found in either the porolepiforms or the osteolepiforms. At present we can therefore only say that the three groups probably evolved from some common pre-Devonian ancestor (Fig. 1).

The incurrent and the excurrent external nostrils in fishes are generally situated in the lateral wall of the snout as are the corresponding openings (the external nostril and the opening of the nasolachrymal duct) in tetrapods. However, in dipnoans and in most sharks (and certain placoderms) the two nasal openings are situated in the ventral wall of the snout. In recent dipnoans (Fig. 7) the excurrent nostril even lies within the cavity of the mouth, a condition which has given rise to the assumption that this nostril represents a true choana. The anatomy and embryologic development of the snout in dipnoans reveals, however, that the two nostrils in dipnoans must correspond to the incurrent and excurrent nostrils of fishes in general and that their unusual position results from a secondary migration downwards and inwards. This migration, which has proceeded a little farther than in the most advanced sharks, has of course brought about marked reductions and other changes in the margin of the upper jaw (*10*). The premaxillary and maxillary bones which in teleostome fishes and tet-

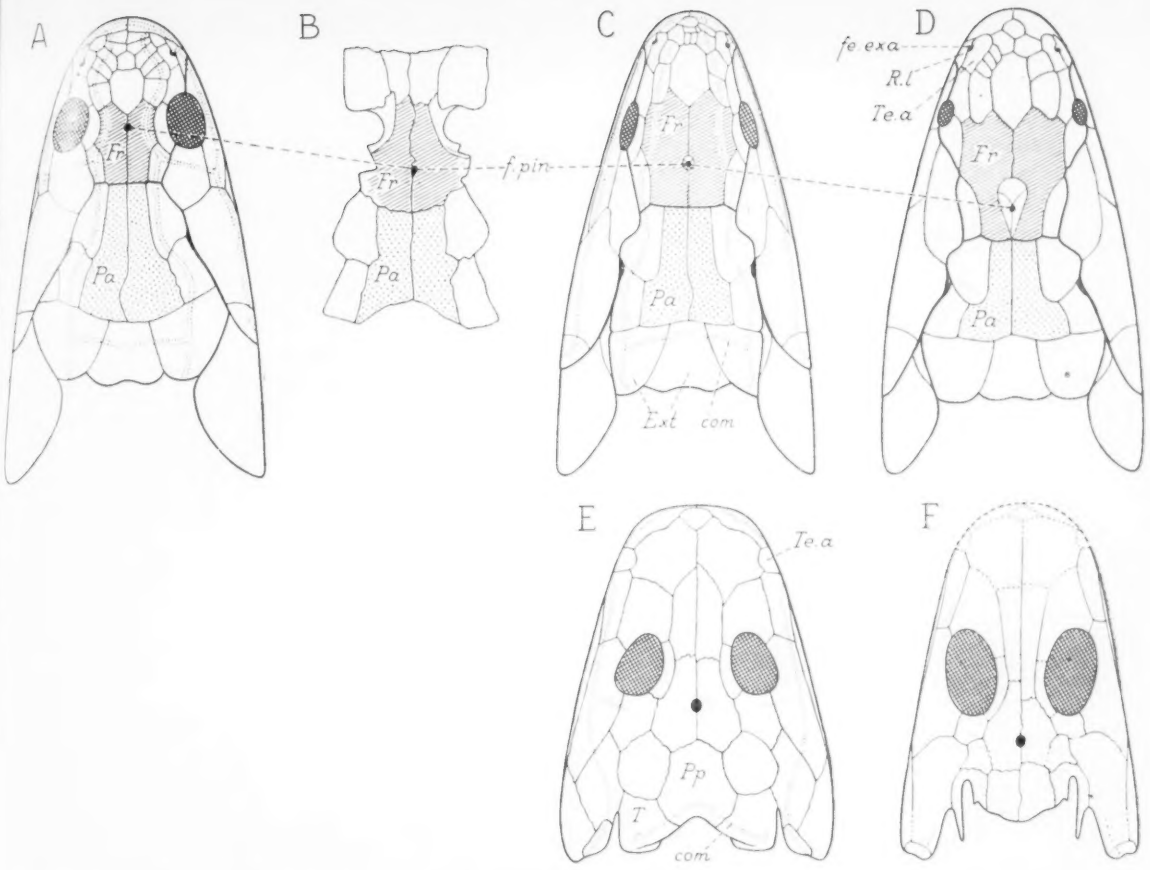

Fig. 4. *A, C, D,* dorsal views of the skull of three Devonian osteolepiforms showing the variations in the relative length of the frontal (hatched) and parietal (stippled) and in the position of the pineal foramen within the Osteolepiformes. *A, Osteolepis macrolepidotus*; Middle Devonian, Scotland. *C, Eusthenopteron foordi*; lowermost Upper Devonian, Scaumenac Bay, Canada. *D, Eusthenodon wängsjöi*; upper Upper Devonian, East Greenland. *B, Elpistostege watsoni*; sketch of fragmentary skull roof of osteolepiform crossopterygian from the same locality (Scaumenac Bay) as *Eusthenopteron foordi*. Ventral aspect. *E, F,* dorsal views of the skull of two early tetrapods (ichthyostegalians); upper Upper Devonian, East Greenland. *E, Ichthyostega* sp.; *F, Acanthostega gunnari. com,* supratemporal commissural sensory canal; *Ext,* extrascapular series of bones; *fe.exa,* opening in exoskeleton for anterior (incurrent) external nostril; *f.pin,* pineal foramen; *Fr,* frontal; *Pa,* parietal; *Pp,* "postparietal"; *R.l,* lateral rostral; *T,* tabular; *Te.a,* anterior tectal. [*A, C,* from Jarvik, 1948 (*11*) and 1944, respectively; *D, E, F,* from Jarvik 1952 (*12*); *B,* after Westoll 1938]

rapods (Fig. 8) form an important part of the upper jaw are lacking, but whether this is the result of reduction or whether the absence is primary cannot be decided with any certainty. The infraorbital sensory canal (Fig. 7), which usually runs forward to the tip of the snout dorsally to these bones, has become reduced, so that together with its nerve, the ramus buccalis, it ends freely in the upper lip outside the region of the posterior nostril. On the other hand, the ramus maxillaris trigemini, which generally accompanies the ramus buccalis and passes outside the true choana, has been pushed medially and runs forward in the roof of the mouth cavity inside the presumed choana.

That the presumed choana in the recent dipnoans represents the posterior external nostril of fishes in general and that it has migrated secondarily into the mouth cavity is further borne out by the conditions in the Devonian forms, where it was still situated outside the mouth cavity (Fig. 7, *C*).

As has been pointed out, the Dipnoi and the Crossopterygii are by most modern students classified in a common systematic unit, the Choanichthyes. Besides, it is generally assumed that the dipnoans are derived from primitive rhipidistids (porolepiforms and osteolepiforms). However, since the choana is primarily absent in both the dipnoans and the coelacanthiforms the name Choanichthyes is clearly misleading and must be rejected. Like the coelacanthiforms, the dipnoans cannot possibly be descendants of any rhipidistids. It is even doubtful that the dipnoans are closely related to the crossopterygians at all. Even in the geologically oldest dipnoans the braincase is fused with the palatoquad-

rate and it does not show any signs whatever of the subdivisions characteristic of the crossopterygians (Fig. 3). In addition, there are numerous important differences between dipnoans and crossopterygians in the snout, in the cranial anatomy, in the pattern of the dermal bones, in the structure of the lower jaw and the palate, in the development of the vertebral column and the ribs (see below and Fig. 9), and so forth. The resemblances used as arguments in order to prove a close relationship between dipnoans and crossopterygians are either the result of misinterpretations of the acutal facts (for example, the supposed choana), or they refer to characteristics shared also by other groups of vertebrates, such as general resemblances in the body form and the presence of two dorsal fins (found in

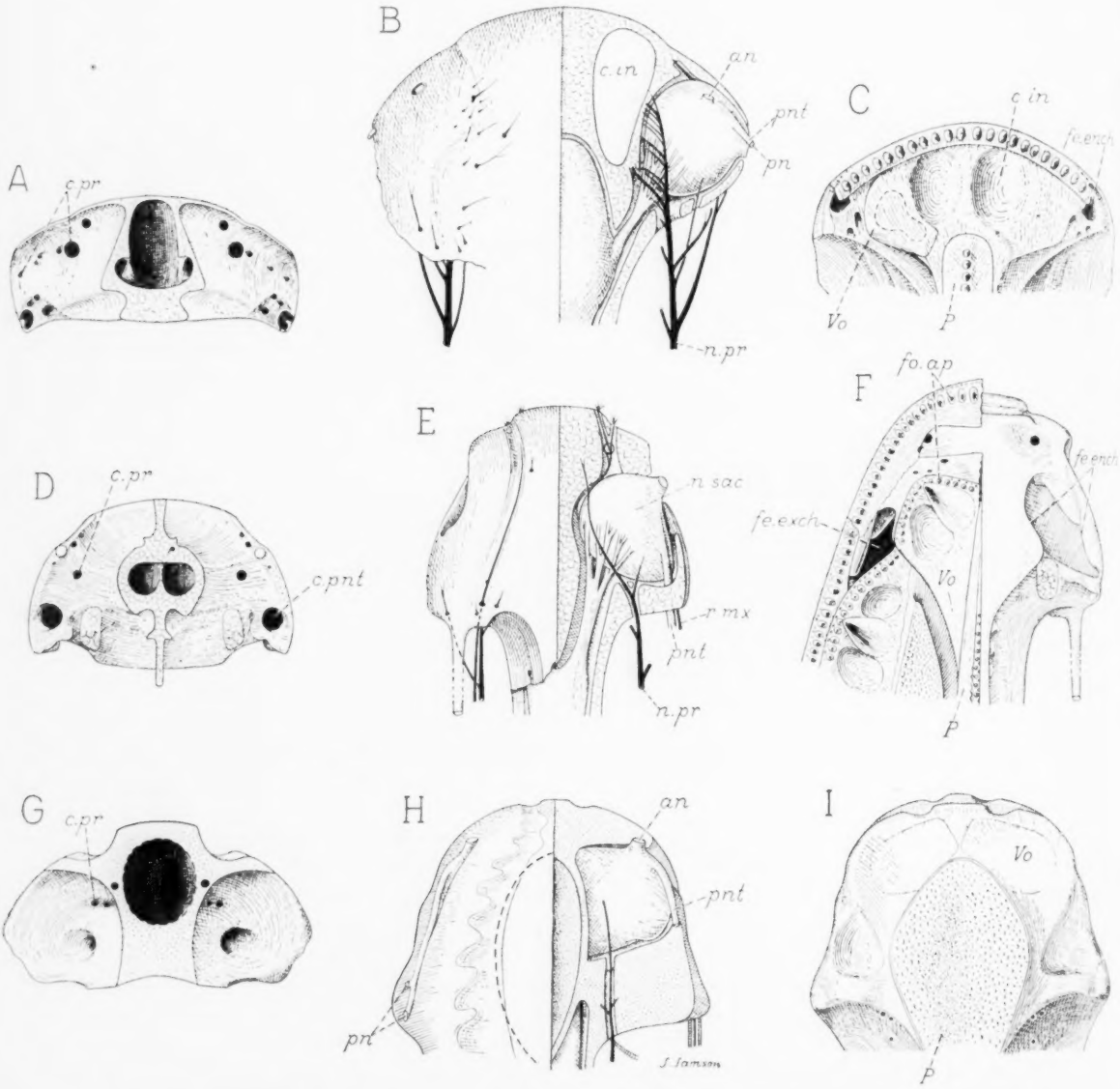

Fig. 5. The anterior part of the head in posterior (left), dorsal (center) and ventral (right) aspects in early crossopterygians, showing the differences among *A–C*, the Porolepiformes (*Porolepis*; Lower Devonian), *D–F*, the Osteolepiformes (*Eusthenopteron*; lowermost Upper Devonian) and, *G–I*, the Coelacanthiformes (*Euporosteus*; lowermost Upper Devonian or upper Middle Devonian). Most of the dermal bones omitted. In *B*, *E*, and *H* the right nasal sac and certain nerves and vessels have been restored. *an*, anterior (incurrent) external nostril; *c.in*, cavum internasale, for intermaxillary gland; *c.pnt*, canal for posterior nasal tube; *c.pr*, canal for nervus profundus; *fe.ench*, opening in endoskeleton for choana (fenestra endochoanalis); *fe.exch*, opening in exoskeleton for choana (fenestra exochoanalis); *fo.ap*, fossa apicalis, for intermaxillary gland; *n.pr*, nervus profundus; *n.sac*, nasal sac (olfactory organ); *P*, parasphenoid; *pn*, posterior (excurrent) external nostril; *pnt*, posterior external nasal tube (nasolachrymal duct); *r.mx*, ramus maxillaris of nervus trigemini; *Vo*, vomer or (in *I*) area marking its position. [From Jarvik 1942 (*10*)]

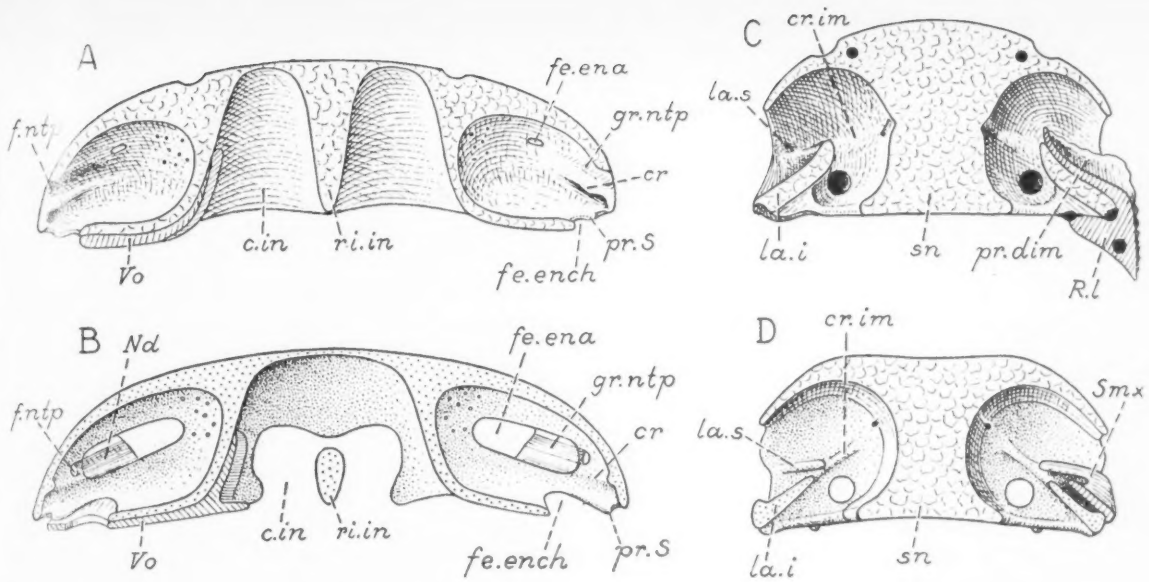

Fig. 6. The anterior part of the snout in posterior views to show the similarities (left) between the Porolepiformes and the Urodela and (right) between the Osteolepiformes and the Anura *A*, the Porolepiform *Porolepis*; Lower Devonian; *C*, the Osteolepiform *Eusthenopteron*; lowermost Upper Devonian; *B,D*, diagrams of a primitive urodele and a primitive anuran. *c.in*, cavum internasale, for intermaxillary gland; *cr*, rostro-caudal crest in postero-lateral part of nasal cavity; *cr.im*, crista intermedia of anterior wall of nasal cavity; *fe.ena*, opening in endoskeleton for anterior (incurrent) external nostril; *fe.ench*, fenestra endochoanalis, for choana; *f.ntp*, opening for posterior nasal tube (naso-lachrymal duct); *gr.ntp*, groove for posterior nasal tube (naso-lachrymal duct) *la.i*, lamina inferior of crista intermedia; *la.s*, lamina superior of crista intermedia; *Nd*, nariodal (endoskeletal ossification earlier confused with the septomaxillary, which is a dermal bone); *pr.dim*, processus dermintermedius of lateral rostral (corresponding to the septomaxillary); *pr.S*, Seydel's palatal process; *ri.in*, internasal ridge or cartilage; *R. l*, lateral rostral; *Smx*, sepotmaxillary, dermal bone, corresponding mainly to the processus dermintermedius of the lateral rostral; *sn*, nasal septum; *Vo*, vomer. [From Jarvik 1942 (*10*)]

ostracoderms, placoderms and elasmobranchs), the presence of lobefins (found, for instance in brachiopterygians and certain actinopterygians) and similarities in the histologic structure of the scales and dermal bones (shared by the ostracoderms). As may thus be seen the assumption that the dipnoans are akin to the crossopterygians is not sufficiently founded. In fact, the dipnoans agree just as much with other groups of vertebrates (placoderms, elasmobranchs, recent amphibians) as with the crossopterygians, but in no case has a close agreement been established. Under these circumstances the systematic position of the dipnoans cannot be settled and the only thing to do for the present is to classify them as a group of their own (Fig. 1).

A paper which more than others has contributed to bringing the dipnoans and the crossopterygians together and which has also had a major influence on the classification of fishes in other respects is Goodrich's well-known description (1907) of the histologic structure of the scales. Goodrich arrived at the conclusion that there is a fundamental difference in the minute structure of the scales between the dipnoans and the crossopterygians, on the one hand, and the actinopterygians, on the other. The scales in *Polypterus* were found to conform most closely with the actinopterygian type and mainly for this reason Goodrich (1928) claimed that *Polypterus* is not at all a crossopterygian but an actinopterygian and a descendant of the paleoniscids.

Since this opinion has been generally accepted it is surprising that in the structure of its head, *Polypterus* differs considerably from the paleoniscids and other actinopterygians in several respects, whereas in many structural details it agrees well with the crossopterygians, particularly with the coelacanthiforms (*10, 13, 17*). As revealed by paleohistologic investigations now in progress in Stockholm, the differences between the crossopterygian and the actinopterygian types of scales are by no means as fundamental as assumed by Goodrich. Moreover, the dermal bones in *Polypterus* and the coelacanthiforms are very similar in external appearance and, in detailed histologic structure, appear to be almost identical. Since, in addition, *Polypterus* differs distinctly from the actinopterygians in its paired lunglike air bladder, in the presence of a well developed epichordal lobe in its caudal fin, and in other respects, it may be doubted

that it is such a good actinopterygian as it is generally maintained to be. Because of their many resemblances to the crossopterygians, in particular to the coelacanthiforms, it would perhaps be most natural to return to the old view and include the brachiopterygians (*Polypterus* and *Calamoichthys*) in the Crossopterygii.

However, there are some differences between the crossopterygians and the brachiopterygians and, besides, new investigations (*10, 13*) have revealed that the structural gap between the crossopterygians and the actinopterygians is hardly as wide as has been assumed. Under these circumstances it seems most reasonable to regard the Brachiopterygii, at least provisionally, as a separate group equal in rank with the Crossopterygii and the Actinopterygii (Fig. 1). It is to be hoped that the detailed anatomical investigations of the living coelacanthids that are now in progress will shed new light on the interrelationships among these three groups and definitively solve the old problem of the systematic position of *Polypterus*.

On the other hand, it is hardly to be expected that future studies of the living coelacanthids will aid materially in elucidating the origin of the tetrapods. In view of the absence of the choana and for numerous other reasons it is out of the question that either the coelacanthiforms, the brachiopterygians, or the dipnoans can have been direct forerunners of the tetrapods. The only possible ancestors of the tetrapods are then the osteolepiform and porolepiform crossopterygians, which are the only fishes possessing the three openings of the nasal sac characteristic of the land vertebrates, that is, the external nostril, the nasolachrymal duct, and the choana.

If we compare the Osteolepiformes and the Porolepiformes with other groups of lower vertebrates we find that exactly the same specializations in the anterior part of the head as in the porolepiforms are met with solely in the urodeles, whereas the numerous specializations characteristic of the osteolepiforms are found in the anurans (Fig. 6). In most cases we meet a close agreement even in the minute details of the internal anatomy between the porolepiforms and the urodeles, on the one hand, and between the osteolepiforms and the anurans, on the other. The main differences in the anterior part of the head between a porolepiform and a salamander or between an osteolepiform and a frog are in fact of secondary importance. More precisely they are mainly the result of the regressive phyletic development of the skeleton.

On account of the very close agreement in the internal anatomy of the anterior part of the head between the osteolepiforms and the anurans it is evident that the Osteolepiformes and the Anura must be very closely related and that in all probability the anurans therefore have evolved from primi-

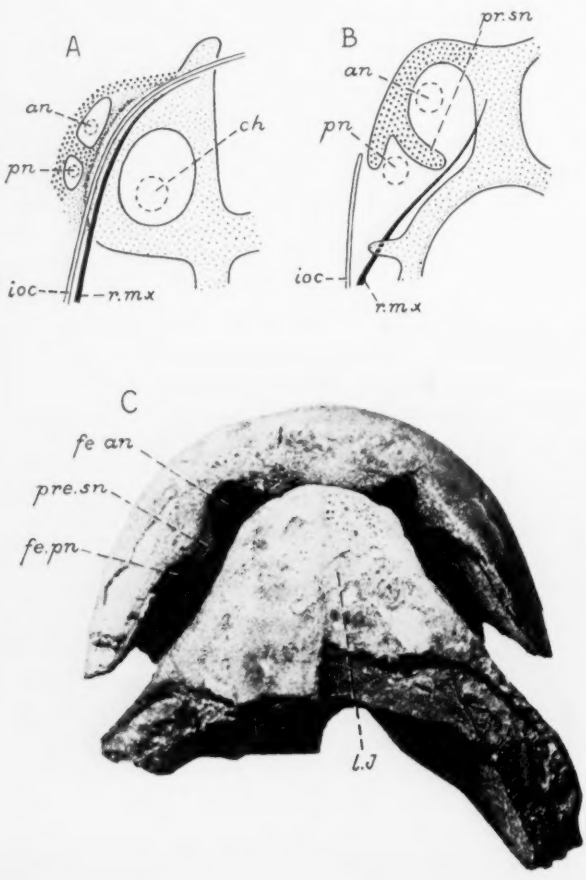

Fig. 7. *A, B,* the position of the nasal openings in, *A,* a choanate gnathostome and, *B,* a dipnoan in relation to the nasal capsule, the infraorbital sensory canal and the ramus maxillaris of the trigeminal nerve. In the modern dipnoans the posterior external nostril and the processus (cartilago) subnasalis, which separates the two nostrils, are situated in the roof of the mouth cavity. The choana is absent in the dipnoans. *C,* the anterior part of the head and the lower jaws in ventral aspect in an early dipnoan (*Chirodipterus wildungensis*; lowermost Upper Devonian). In contrast with modern dipnoans the processus subnasalis and the opening for the posterior external nostril are still situated outside the margin of the lower jaw. The processus subnasalis is ossified and is covered ventrally by a similar process formed by the exoskeleton. The latter process, like other parts of the external exoskeleton, is coated by a shiny layer of dentine and enamel. *an,* anterior (incurrent) external nostril; *ch,* choana (internal nostril); *fe.an, fe.pn,* openings for anterior and posterior external nostrils; *ioc,* infraorbital sensory canal; *l.j,* lower jaws; *pn,* posterior (excurrent) external nostril; *pre.sn,* exoskeletal subnasal process (endoskeletal process concealed); *pr.sn,* endoskeletal subnasal process (cartilago subnasalis); *r.mx,* ramus maxillaris trigemini. [*A, B,* from Jarvik 1942 (*10*); *C,* specimen belonging to the Paleontological Institute and Museum of the University of Berlin]

tive osteolepiforms (*10*). This conclusion is strongly supported by the fact that several of the structural features characteristic of adult anurans arise comparatively late during the embroylogic development from an osteolepiformlike initial stage of development. For the same reasons it must be concluded that the Urodela are closely allied to the Porolepiformes and are derived from the latter group. The resemblances existing between the recent amphibians go back to similarities between the ancestral fish groups or arose independently in the osteolepiform-anuran and the porolepiform-urodele lines of evolution (Figs. 1, 10).

Now that we have seen that the urodeles and the anurans are descendants of two different groups of fishes, the tetrapods thus being diphyletic, the question arises from which of these two groups the other tetrapods developed. As will be demonstrated, the Devonian ichthyostegalians are certainly descendants of osteolepiforms; that the same is true at any rate of most post-Devonian labyrinthodonts is evidenced, among other things, by the structure of the palate and the presence and development of the septomaxillary (lateral rostral). Since a septomaxillary of the osteolepiform type is found also in primitive reptiles and mammals, and for other reasons, we may be sure that the Amniota, too, are derived from osteolepiforms. The Osteolepiformes have thus given rise to the vast majority of the Tetrapoda (Figs. 1, 10). It is not unthinkable, however, that certain imperfectly known stegocephalians (and the Apoda?) may belong to the porolepiform-urodele stock. This applies above all to those stegocephalians in which the parasphenoid is of the broad type characteristic of the porolepiforms (*10*).

As may be gathered from Fig. 8, the anterior part of the palate in the Triassic labyrinthodont

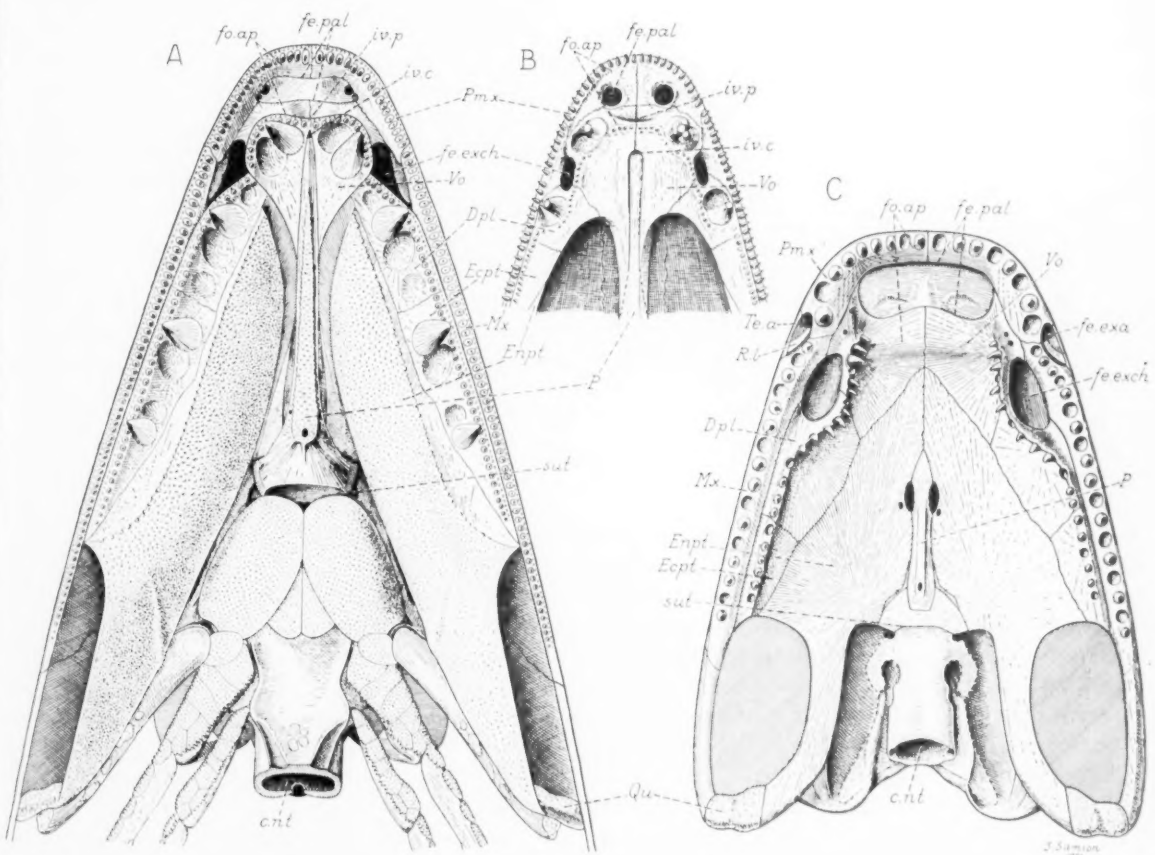

Fig. 8. Ventral views of the palate of, *A*, an osteolepiform crossopterygian (*Eusthenopteron foordi*), *B*, a Triassic stegocephalian (*Mastodonsaurus*) and, *C*, a Devonian stegocephalian (*Ichthyostega*). *c.nt*, canal for notochord; *Dpl*, dermopalatine; *Ecpt*, ectopterygoid; *Enpt*, entopterygoid; *fe.exa*, opening for anterior external (incurrent) nostril; *fe.exch*, opening in exoskeleton for choana; *fe.pal*, anterior palatal fenestra; *fo.ap*, fossa apicalis, for intermaxillary gland; *iv.c*, *iv.p*, intervomerine canal and intervomerine pit, for efferent ducts of intermaxillary gland; *Mx*, maxillary; *P*, parasphenoid; *Pmx*, premaxillary; *Qu*, articular area for the lower jaw; *R.l*, lateral rostral; *sut*, limit between anterior and posterior ossifications of braincase; *Te.a*, anterior tectal; *Vo*, vomer. [From Jarvik 1954 (*13*), 1942 (*10*), and 1952 (*12*)]

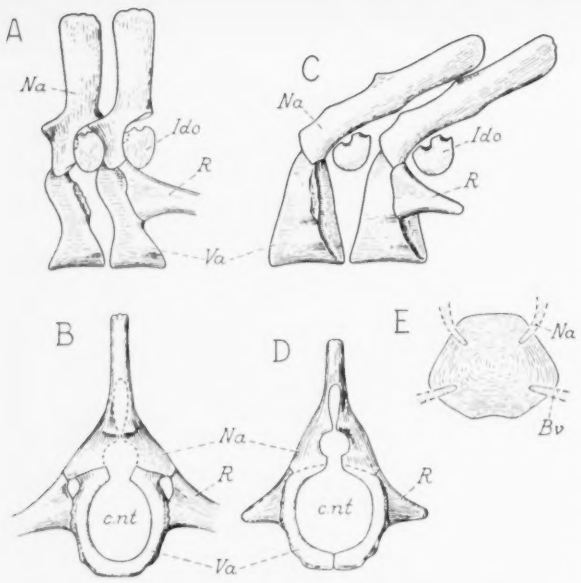

Fig. 9. *A–D*, two vertebrae of (left) an ichthyostegid stegocephalian (*Ichthyostega*) and (right) an osteolepiform crossopterygian (*Eusthenopteron*) in lateral and anterior aspects. *E*, vertebra of early dipnoan in transverse section; upper Upper Devonian, East Greenland. The cartilaginous basal parts of the arcual elements restored. *B.v*, basiventral; *c.nt*, canal for notochord; *Ido*, interdorsal; *N.a*, neural arch (basidorsal); *R*, rib; *V.a*, ventral vertebral arch. [From Jarvik 1952 (*12*)]

Mastodonsaurus is very suggestive of that in the osteolepiform *Eusthenopteron* and even such specialized features as the intervomerine pit and the intervomerine canal in the posterior wall of the fossa apicalis have been retained without any marked changes. Similar conditions are found in most other Triassic stegocephalians (capitosaurids, benthosuchids, metoposaurids, and so forth), and evidently all these late stegocephalians therefore evolved from an osteolepiform in which, as in *Eusthenopteron*, the fossa apicalis is well developed. From this it follows that the late stegocephalians can hardly be descendants of any of the well-known paleozoic forms, for all these early stegocephalians are without the intervomerine canal and the intervomerine pit (Fig. 8, *C*) or they differ in other respects more from *Eusthenopteron* than the Triassic forms mentioned above.

These and other conditions (*10*) raise some doubt about the validity of the evolutionary lines generally distinguished within the Labyrinthodontia and lead us to the conclusion that *Mastodonsaurus* and its allied forms arose from the osteolepiforms independently of all the paleozoic stegocephalians known in any detail. Neither have there been found any unquestionable paleozoic transitional forms between the osteolepiforms and the anurans, or between the osteolepiforms and the reptiles, and for these and other reasons it is likely that the osteolepiforms have given rise to several evolutionary lines reaching the tetrapod stage. This would mean that the tetrapods are polyphyletic (Fig. 10).

The oldest tetrapods. The ichthyostegalians, which are the first land vertebrates to appear in the geological record and the only Devonian tetrapods hitherto found, are known only from the late Devonian Old Red Sandstone at Franz Joseph Fjord (latitude 73° + 74° N.) in East Greenland. They were not discovered until 1931 when, during the Danish East Greenland expedition under the leadership of Lauge Koch, G. Säve-Söderbergh (*14*) and his assistants found some imperfect skulls on Mount Celsius on Ymer Island. This discovery naturally prompted further search and since 1931 Koch's expeditions have brought home a large collection (about 220 specimens) of these interesting animals both from Mount Celsius and from the Gauss Peninsula on the opposite side of Franz Joseph Fjord, where rich beds were discovered in 1932. This material is now in Stockholm for preparation and description, and the preparation has proceeded so far that it is possible to give—for the first time—a restoration of the skeleton of a Devonian tetrapod (Fig. 11).

On the basis of the material obtained in 1931 Säve-Söderbergh (*14*) distinguished two genera, *Ichthyostega* and *Ichthyostegopsis*, which he referred to the family Ichthyostegidae, forming the order Ichthyostegalia. The new material, which so far has been only partly described (*12*), contains ichthyostegid skulls of various types; probably one or two more genera are represented. Besides, there are a few incomplete skulls of another stegocephalian, *Acanthostega*. The latter form differs so much from the ichthyostegids in the posterior part of the skull (Fig. 4, *E, F*) that it must be placed in a family of its own, the Acanthostegidae, which only provisionally can be included in the Ichthyostegalia.

The Upper Devonian tetrapod fauna of East Greenland is thus rich in forms, and the presence of types so different as the ichthyostegids and the acanthostegids demonstrates that the radiation of the Tetrapoda was well under way during this time. Since, in the posterior part of the skull, the ichthyostegids and the acanthostegids have become specialized in different directions they must have developed independently of each other for a considerable time and it is not unlikely that they arose separately from different osteolepiforms.

The ichthyostegids agree in many respects closely with the osteolepiforms. The general pattern of the dermal bones in the anterior and lateral parts of

the snout, in the cheek and in the external side of the lower jaw is thus identical in the two groups (Fig. 12). There is exactly the same number of dermal bones, and the bones have the same relations to the sensory canals and, on the whole, to each other as well. Two of these bones, the anterior tectal (often confused with the lateral rostral or septomaxillary) and the preoperculum are lacking in all post-Devonian stegocephalians. Behind the cheek in the ichthyostegids there is a distinct but reduced suboperculum situated the same as that in the osteolepiforms. This bone is also lacking in later tetrapods and its presence in the ichthyostegids can only mean that a gill cover persisted in front of the cleithrum. However, no traces of gill arches (or stapes) have been found and conceivably these structures were weakly developed and partly cartilaginous, just as they are in the osteolepiforms. Another remarkable piscine feature retained in the ichthyostegids is that the sensory lines are enclosed in the dermal bones.

The lower jaw, too, is very suggestive of that in the osteolepiforms. The agreement is, in fact, so close that it is often difficult to decide whether the numerous detached jaws obtained from the late Devonian of East Greenland pertain to an ichthyostegid or to the osteolepiform (*Eusthenodon*, *12*) occurring in the same deposits. Also in the palate the ichthyostegids in most respects agree well with the osteolepiforms (Fig. 8). Moreover, it is noteworthy that the parasphenoid of the ichthyostegids is short, as in the crossopterygians, extending backward only as far as the region of the hypophysis. This resemblance is owing to the important fact that the ichthyostegids are the only tetrapods in which the transverse subdivision of the neural endocranium characteristic of the crossopterygians is still traceable (the braincase shows a transverse suture close behind the fossa hypophyseos).

The transverse suture between the frontoethmoidal and parietal shields of the osteolepiforms, on the other hand, has disappeared in the ichthyostegids and the cheeks have fused with the skull roof (Fig. 4). In connection with these modifications, which are foreshadowed in the osteolepiforms, and other modifications caused by fusions of dermal bones (*11–13*), the skull roof has changed so radically that all attempts have failed to homologize the comparatively few dermal bones in the ichthyostegids (and other lower tetrapods) with the numerous bones in the corresponding position in the osteolepiforms. Similar changes have taken place in the neck region of the ichthyostegids, and, as evidenced for example by the course of the supratemporal commissural sensory canal (Fig. 4) the bones of the extrascapular series of the osteolepiforms have become incorporated into the skull roof of the ichthyostegids, entering into the formation of the tabular and postparietal.

In papers discussing the differences in the skull roof of the crossopterygians and of the stegocephalians, reference has often been made to a fragmentary skull roof from the lowermost Upper Devonian of Scaumenac Bay, Canada, incompletely described under the generic name of *Elpistostege*. On the basis of this skull roof (Fig. 4, *B*) and of the position of the pineal foramen in various forms, attempts have been made to determine the homologies of the dermal bones.

It has also been maintained that, in the proportions of the skull roof, *Elpistostege* is intermediate between osteolepiforms and ichthyostegids, and seemingly convincing diagrams illustrating this opinion have been published. It should be noticed, however, that the skull roof varies considerably within the Osteolepiformes (Fig. 4 *A–D*) both with regard to its proportions and the position of the pineal foramen. Thus, in *Osteolepis* the frontal is much shorter than the parietal and the pineal foramen is situated far forward. In *Eusthenopteron foordi*, which is from the same locality (Scaumenac Bay) as *Elpistostege*, the frontal and parietal are equal in length and the pineal foramen lies a little behind the middle of the frontals. In *Eusthenodon*, finally, the frontal is longer than the parietal and the pineal foramen is found far back. As may be

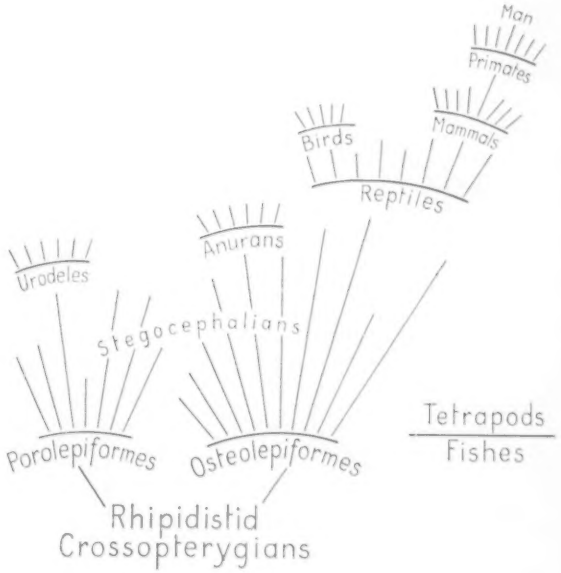

Fig. 10. The polyphyletic origin of the Tetrapoda and the importance of the osteolepiform crossopterygians as a stem group of the vast majority of the land vertebrates.

gathered from Fig. 4, *Elpistostege* is, in these respects, exactly like *Eusthenopteron foordi*. It is thus by no means intermediate between osteolepiforms and ichthyostegids.

The ichthyostegids differ from the osteolepiforms in the position of the external nostril (Figs. 4, 8, 12). In the ichthyostegids this opening, which corresponds to the anterior (incurrent) nostril of fishes, has secondarily shifted downward; this migration has caused changes in the upper jaw similar to those in the dipnoans and certain sharks. The lateral rostral has been reduced to a thin cylinder carrying the infraorbital sensory canal and this canal and the adjoining ends of the maxillary and premaxillary have shifted inward, so that the sensory canal forms a curve inside the nostril as in certain advanced sharks. Since the margin of the upper jaw is intact in later tetrapods, as it is in all osteolepiforms (Fig. 8), the ichthyostegids cannot be ancestral to any other tetrapods so far known. For this and many other reasons it must be concluded that the ichthyostegids represent a blind branch of the osteolepiform-labyrinthodont stock (Fig. 1).

In the postcranial skeleton, striking similarities are found, too, between the ichthyostegids and the osteolepiforms (*12*). This applies particularly to the vertebral column. The individual vertebras are almost identical in structure (Fig. 9), consisting of two dorsal arcual elements, the interdorsal and the basidorsal or neural arch, and a large ventral element, probably formed by the fusion of an interventral with a basiventral. The ribs are dorsal ribs with a bifid proximal articular head as in tetrapods in general. However, the thoracic ribs of the ichthyostegids have become much broadened and form a cuirass around the body cavity. In the early dipnoans, which previously have been considered to be closely related to the osteolepiforms and ancestral to tetrapods, the vertebral column is fundamentally different (Fig. 9, *E*). In these it consists of solid centra partly formed within the notochordal sheet as in sharks. These centra exhibit deep pits for the cartilaginous basal parts of the arcual elements. In addition, the ribs are of the pleural (ventral) type.

The appendicular skeleton in the ichthyostegids (Fig. 11) is developed fundamentally as that in other stegocephalians and primitive reptiles and they have typical pentadactyl limbs. However, in several respects the appendicular skeleton of the ichthyostegids is more primitive and fishlike than that of the later tetrapods, approaching that of the osteolepiforms. Studies of *Eusthenopteron* now going on have revealed that the skeleton of the paired fins and their girdles in the osteolepiforms had become specialized in such a way that a transformation into a typical tetrapod limb could take place without any great structural changes. These specializations are clearly seen, for example, in the proximal element of the pectoral fin, which, in spite of the fact that the pectoral fin in *Eusthenopteron* has long been a favored object in the speculations about the origin of the tetrapod limb, has hitherto remained practically unknown. In fact, this element has acquired most of the processes and other

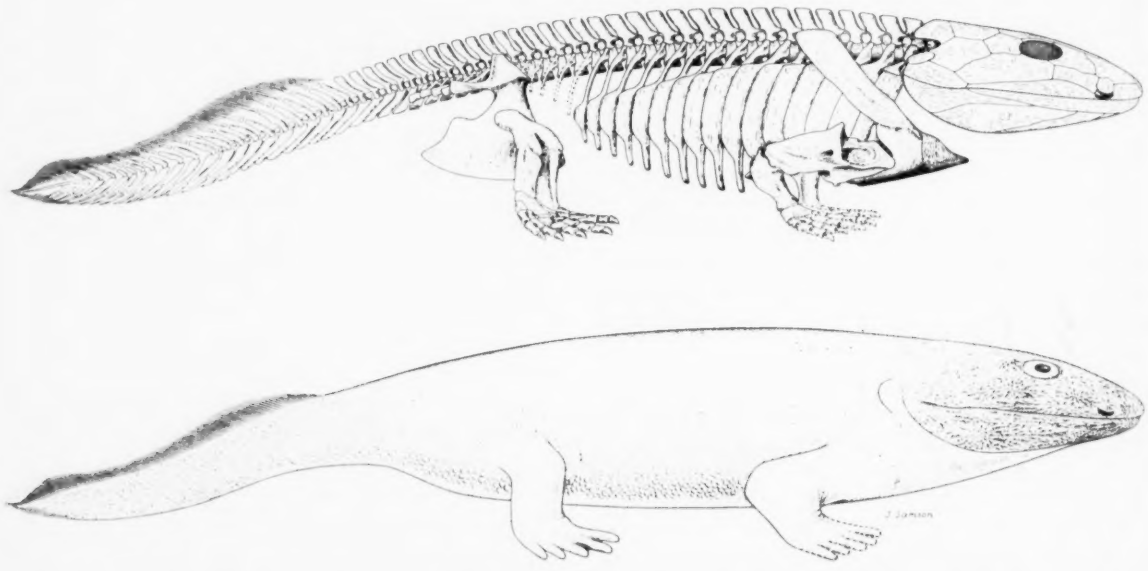

Fig. 11. Restorations of the oldest tetrapod, *Ichthyostega,* from the upper Upper Devonian of East Greenland; about 65 cm long. The large specimens probably attained a length of about 1 m.

structural features distinctive of the primitive tetrapod humerus and consequently has to be regarded as a true humerus. It is now clear that the tetrapod limb, at any rate in the descendants of the osteolepiforms, arose from a pectoral fin of the type found in *Eusthenopteron* without addition of a "neopodium" or other profound changes.

Finally, the tail of the ichthyostegids (Fig. 11) is decidedly of the fish type, for, unlike the tail in any other tetrapod, it is provided with a true fin supported by both endoskeletal radials and dermal fin rays (lepidotrichia). This fin represents chiefly a caudal fin, but the fact that dorsally to the body axis it is clearly subdivided into two portions, anterior and posterior, must certainly mean that it also includes a posterior dorsal fin. In all probability it has therefore arisen by the fusion of a posterior dorsal fin with the dorsal lobe of a heterocercal caudal fin of the type found in *Osteolepis* and many other osteolepiforms. Exactly as in osteolepiforms, the ventral lobe of the heterocercal caudal fin in the ichthyostegids extends farther forward than the dorsal lobe, but, unlike in the osteolepiforms, its principal anterior part lacks dermal fin rays and is represented by somewhat modified radials. This latter condition is very likely the result of the fact that this part of the tail dragged on the ground when the animal was moving in shallow water or on land (*12*).

The presence of pentadactyl limbs and the general structure of the appendicular skeleton prove that the ichthyostegalians have definitely reached the tetrapod stage of evolution and for these and other reasons they are to be regarded as true tetrapods. With regard to the structure of the tail and in numerous other respects, on the other hand, they are fundamentally fishes or are intermediate between fishes and later tetrapods. The ichthyostegalians, or the four-legged fishes—as they have been called with some justification—thus aid considerably in elucidating the structural changes that took place during the important phase of the evolutionary history of the vertebrates from fish to tetrapod.

It must be pointed out, however, that in view of the modern paleoanatomical investigations of the early crossopterygians the structural gap between fishes and tetrapods is not nearly as wide as previously supposed. Thus, in the rhipidistid crossopterygians, as we have seen, numerous structural characteristics distinctive of tetrapods are found (Fig. 6). Several of these characteristics, for example the choana and in all probability the lungs as well, must have been present in the common ancestors of the porolepiforms and the osteolepiforms; the origin of these basic characteristics started the

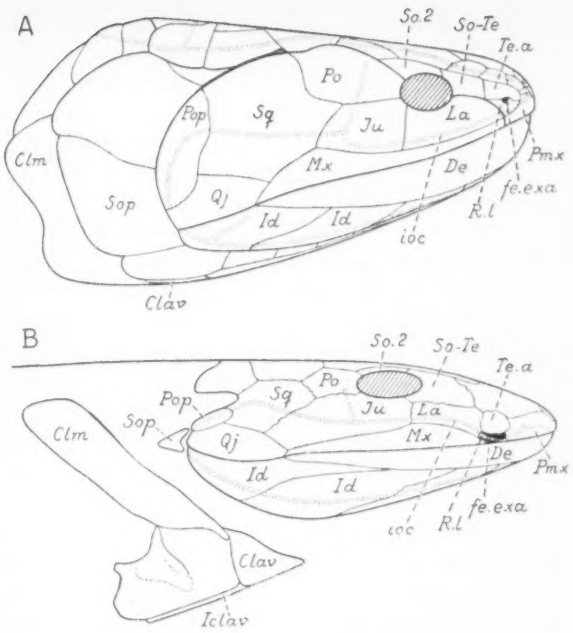

Fig. 12. The skull and the shoulder girdle in lateral aspects in, *A*, an osteolepiform fish (*Eusthenopteron foordi*) and an early tetrapod (*Ichthyostega*; Upper Devonian, East Greenland) showing the agreements in the general pattern of the dermal bones and in the course of the sensory canals. *Clav*, clavicula; *Clm*, cleithrum; *De*, dentary; *fe.exa*, opening in exoskeleton for anterior (incurrent) external nostril; *Id*, infradentaries; *ioc*, infraorbital sensory canal; *Ju*, jugal; *La*, lachrymal; *Mx*, maxillary; *Pmx*, premaxillary; *Po*, postorbital; *Pop*, preoperculum; *Qj*, quadratojugal; *R.l*, lateral rostral; *Sop*, suboperculum; *So-Te*, supraorbito-tectal (prefrontal); *So.2*, posterior supraorbital (postfrontal); *Sq*, squamosum; *Te.a*, anterior tectal. [*A*, from Jarvik 1944; *B*, from Jarvik 1952 (*12*), with addition of the shoulder girdle]

evolution of the tetrapods in the Silurian or earlier. When, later on, the porolepiforms and the osteolepiforms became separate groups, a great number of additional specializations arose, in the porolepiforms of the urodele type and in the osteolepiforms of the anuran type. In the porolepiforms such specializations have been demonstrated mainly in the anterior part of the head (*10*). In the osteolepiforms typical tetrapod features have been recognized in other parts as well (*10*, *13*), and, for instance, the skeleton of the paired fins has acquired many specialized features distinctive of the limb skeleton in tetrapods. By these changes the osteolepiforms and the porolepiforms had, even at the time of their first appearance more than 300 million years ago, reached such an evolutionary stage that a transition to a terrestrial mode of life could take place without any profound changes in internal anatomy.

The glimpses of the Devonian tetrapod life given us by the East Greenland ichthyostegalians are of course most incomplete. The diversity of

types among the later tetrapods, none of which may be derived from the known Devonian forms, shows that a rich tetrapod fauna including several evolutionary lines existed in late Devonian time. It is difficult to imagine that all these lines arose from one single porolepiform and one single osteolepiform. We may therefore assume that the comparatively small structural changes needed to convert a porolepiform or an osteolepiform fish into a tetrapod took place several times in each of these groups of fishes and that accordingly the tetrapods are polyphyletic (Figs. 1, 10).

If we compare the recent and other late members of the various evolutionary lines within the Amphibia with their ancient predecessors among the porolepiform and osteolepiform fish, it is surprising to find how small have been the structural changes and how slow the rate of evolution in many cases. Thus the recent urodeles and anurans in many respects agree most closely with their Devonian forerunners, and in numerous features they have undergone but small modifications in their internal structure, or they have not changed at all, since the very remote times when the basic specializations characteristic of each of these groups arose.

In the stegocephalians, too, we often meet a similar conservatism, and even the reptiles and mammals have retained many features inherited from their ancestors among the rhipidistid crossopterygians. In order to fully understand the structure of their skeleton and related soft parts, to determine which structures are primitive and which are secondary acquisitions, to explain the processes occurring during their embryologic development, and so forth, in amphibians and in tetrapods in general, it is therefore necessary to be thoroughly familiar with the ancestral fishes. An intimate knowledge of the rhipidistid crossopterygians is accordingly indispensable for all students interested in the phyletic and ontogenetic evolution of the tetrapods, their mutual relationships, and classification.

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Grandeur consists in form, and not in size; and to the eye of the philosopher, the curve drawn on a paper two inches long is just as magnificent, just as symbolic of divine mysteries and melodies, as when embodied in the span of some cathedral roof.— CHARLES KINGSLEY.

Sherlock Holmes as an Anthropologist

WILTON MARION KROGMAN

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SHERLOCK HOLMES, in speaking of François de Villard, the French detective, said: "He possesses two out of the three qualities necessary for the ideal detective. He has the power of observation and that of deduction. He is wanting only in knowledge . . ." (p. 93).* Holmes possessed all three traits. His knowledge was extensive and in many instances intensive. Among the fields in which he showed considerable knowledge was anthropology. Primarily, his interests were concerned with what we today classify as physical anthropology, but he knew archeology and ethnography as well. I shall discuss the evidence under these headings.

Physical Anthropology

General background. In *A Study in Scarlet* Holmes' "limits" are given. There are twelve categories, the eighth of which is anatomy. Watson rates him here as "accurate, but unsystematic" (p. 11). It must be remembered that the British anatomy courses of the last half of the 19th century contained much physical anthropology. At that time there was no physical anthropology, per se. At that time, too, anatomy was gross anatomy, that is, morphologic as opposed to histologic. It was only natural, therefore, for anatomists to include physical anthropology in their thinking, teaching, and occasional research. In *The Hound of the Baskervilles* Holmes refers to a day spent at the Museum of the College of Surgeons for "pure amusement" (p. 818). It is quite likely that here he studied the Flower and the Hunterian collections of comparative and human osteology.

Estimation of stature. Of all reconstructions or deductions involving the human body, the calculation of height seemed to have concerned Holmes the most. As a rule he used stride and foot size as criterions, although on at least one occasion he used his own stature as a gage. In *The Sign of the Four* Holmes said that Jonathan Small's "height is readily calculated from the length of his stride (p. 129). Moreover, in *The Boscombe Valley Mystery* Watson, in following Holmes' deductions, says to him: "His height I know that you might roughly judge from the length of his stride" (p. 240). Nowhere are we told precisely how all this is done. In fact, Holmes deliberately refrains from doing so. In *A Study in Scarlet* he says to Watson: "Why, the height of a man, in nine cases out of ten, can be told from the length of his stride. It is a simple calculation enough, though there is no use my boring you with figures" (p. 24).

But Holmes does give us a lead. In the same case he describes the probable murderer as "more than six feet high" (p. 23). In part, this was deduced from the fact that his stride was "four and half feet" (p. 24). We are led to assume that the stride is about two-thirds, or a little more, of the total stature. In this case, Holmes said: "I had a way of checking my calculation. When a man writes on a wall, his instinct leads him to write above the level of his own eyes. Now that writing [the word *Rache*] was just over six feet from the ground. It was child's play" (p. 24).

On occasion, foot size (length) was used in estimating stature or in analyzing footprints at the scene of a crime. In *A Study in Scarlet* Holmes averred that the murderer "had small feet for his height" (p. 23). In *The Singular Experience of Mr. Scott Eccles* Holmes remarked of a footprint: "a number twelve shoe, I should say. If he was all on the same scale as his foot he must certainly have been a giant" (p. 1032). In *The Boscombe Valley*

* In presenting the data I shall refer to source and quote by page number. I have used *The Complete Sherlock Holmes,* vols. 1 and 2, by Sir Arthur Conan Doyle; preface by Christopher Morley (Doubleday, New York, 1953).

Mystery Holmes measured the boots of Charles and James McCarthy, father and son, "very carefully from seven or eight different points" (p. 236).

In *The Adventure of the Three Students* height is a factor in the solution of the mystery of the copied examination papers. In this case Holmes refers to "unusual height" (p. 707) and "great height" (p. 708). He asked Mr. Soames how tall Miles McLaren was, and was told, "I suppose five foot six would be about it." We are told that Gilchrist was "a tall, flaxen-haired, slim young fellow" (p. 703). It was he who was the culprit, for only he, of the three, was tall enough to see the proof sheets upon the tutor's desk. Holmes had deduced that the height of the window above the ground precluded a short-statured person from peering into the room.

In *The Adventure of the Abbey Grange* Holmes deduced the murderer as being "six foot three in height" (p. 753). This he did by climbing up on a mantel piece, bracing his knee on a bracket, and trying to reach the level where a bell cord had been severed by a knife. Holmes says, "I could not reach the place by at least three inches—from which I infer that he is at least three inches a bigger man than I" (p. 754). [Parenthetically, this makes Holmes a six-footer, but Watson describes him as "rather over six feet" (p. 9).]

On one occasion Holmes used length of stride in a different manner. In *The Crooked Man* he noticed "four prints where the beast had been standing motionless" (p. 480). He observed that the distance from forefoot to hindfoot was 15 in.; add to this length of head and neck, and over-all length is about 24 in. Yet, the stride is only 3 in.; this gives "a long body with very short legs attached to it" (p. 480). The animal was later revealed as a mongoose, or ichneumon (p. 486).

Holmes even used stride-length to assess behavior. In *A Study in Scarlet* he observed of the murderer: "as he walked he grew more and more excited. That's shown by the increased length of his strides" (p. 26.)

In two cases Holmes had to contend with a wooden leg. Even so, he felt that the length of the stride warranted an assumption of stature. The murderer in *The Sign of the Four* is deduced "as a poorly educated man, small, active with his right leg off, and wearing a wooden stump . . ." (p. 121). In *The Adventure of the Beryl Coronet,* the greengrocer who stood to the left of the door was wooden-legged (p. 354). Here, however, only his presence was of moment.

Feet and hands. The feet figure in several cases, apart from deduction of stature. In *The Sign of Four* there is reference to the fact that "the floor was covered thickly with the prints of a naked foot—clear, well-defined, perfectly formed, but scarce half the size of those of an ordinary man" (p. 118). When Holmes printed his own right foot beside a comparable print on the floor, it was noted by Watson that "your toes are all cramped together. The other print has each toe distinctly divided" (p. 125). Later on, Holmes summarizes, in part: "Diminutive footmarks, toes never fettered by boots, naked feet . . ." (p. 136). Watson says: "A savage! Perhaps one of those Indians who were the associates of Jonathan Small!" To which Holmes replies: "Some of the inhabitants of the Indian Peninsula are small men, but none could have left such marks as that. The Hindoo proper has a long and narrow foot. The sandal-wearing Mohammedan has the great toe well separated from the others because the thong is commonly passed between" (p. 136). Holmes deduced correctly that the maker of the prints was an Andamanese—"the average height is rather below four feet . . . feet and hands . . . remarkably small . . ." (p. 137).

The hand also figures frequently in Holmes' deductions, but almost exclusively with reference to occupational stigmata. I shall not consider these in this article. In *The Red-Headed League* he noted use hypertrophy in Mr. Wilson's hands when he observed that "your right hand is quite a bit larger than your left. You have worked with it, and the muscles are more developed" (p. 196). In *The Yellow Face* Holmes deduced from a smoking pipe that the owner was left-handed because the right side of the pipe was charred from holding it to a lamp flame (p. 403). In *The Boscombe Valley Mystery* Holmes describes the murderer as "a tall man, left-handed . . ." (p 238). The left-handedness was deduced from the nature of the head wound sustained by the victim; the left parieto-occipital area was involved. "The blow was struck from immediately behind, and yet was on the left side. Now, how can that be unless it were by a left-handed man?" (p. 240)

The skull. There is surprisingly little reference to cranial (cephalic) type in Holmes' cases. Indeed, Dr. Mortimer, who brought *The Hound of the Baskervilles* to Holmes' attention, is the one primarily concerned. When he first met Holmes his earliest remarks were: "I had hardly expected such a dolichocephalic skull or such well-marked supraorbital development. Would you have any objection to my running my fingers along your parietal fissure? A cast of your skull, sir, until the original is available, would be an ornament to any anthropological museum. It is not my intention to be fulsome, but I confess that I covet your skull" (p. 787). Later on Holmes says to Dr. Mortimer, "I

presume. Doctor, that you could tell the skull of a Negro from that of an Esquimaux?" (p. 803) (The fact that a doctor in the late 1800's is presumed to know racial craniology is prima facie evidence of the then close relationship between anatomy and anthropology. This is in the Hunterian and Cunningham traditions). Dr. Mortimer replied in the affirmative, listing such criterions as "the supraorbital crest, the facial angle, the maxillary curve . . ." (p. 803). In his first report to Holmes, Watson wrote that Dr. Mortimer "has been excavating a barrow at Long Down and has got a prehistoric skull which fills him with great joy" (p. 836).

Skeletal material. It is noteworthy that in all his cases Holmes encountered the skeleton, other than the skull, only three times—and in each instance the bones were burned! In *The Adventure of the Norwood Builder*, charred remains were found in a burned woodpile (p. 578). Holmes went to the scene at Deep Dene House. Although he does not say so in so many words, he apparently accepted without challenge the "charred organic remains" (p. 584) as possibly human. He studied the burned trouser buttons carefully. The whole examination he called a "fiasco," in the sense that no new leads were uncovered. Surely, if he had identified the bones as nonhuman he would have earlier come to the conclusion that Jonas Oldacre had *not* been burned in the woodfire. Not until Oldacre had been flushed from his hiding place did Holmes use the term *animal remains* (p. 592). In a jovial mood, after Oldacre had revealed his plot, Holmes asked: "By the way, what was it you put in the woodpile besides your old trousers? A dead dog, or rabbits, or what? . . . I daresay that a couple of rabbits would account for the blood and the charred ashes" (p. 593).

In *The Adventure of Wisteria Lodge* Inspector Baynes showed Holmes "a platter heaped with small pieces of burned bones" (p. 1033). When asked by the Inspector, "What do you make of these bones?" Holmes instantly replied, "a lamb, I should say, or a kid" (p. 1033). He did not say how he knew this; it may be that he made a cultural, rather than an osteologic, deduction, for at the conclusion of the case he cited the sacrifice of a black goat in voodoo worship (p. 1043).

Burned bones figure also in *The Adventure of Shoscombe Old Place*. Mr. John Mason stated that he and Stephens had gone down into the crypt and found "the head and a few bones of a mummy" (p. 1303). Later Mr. Mason produced "a charred fragment of bone" taken from "a central heating furnace in the cellar below Lady Beatrice's room" (p. 1304). Even though the bone "was burned to a black cinder there could be no question of its anatomical significance." Without hesitation Holmes stated it to be "the upper condyle [*sic*] of a human femur" (p. 1304). The use of the term *upper condyle* is, of course, incorrect. There is no such anatomical structure. It may be that upper *part* of the femoral condyle was meant; it may be that, in the knee, femoral condyles were "upper" and tibial condyles were "lower," much as "superior maxillary" (maxilla, per se) and "inferior maxillary" (mandible) were used. At all events, identification was either wholly incorrect, or loosely stated.

It is difficult to give a precise figure, but it is not too far from wrong to say that more than 90 percent of Holmes' deductions were based upon the observation of the living human body.

Race and racial traits. These loom quite importantly in Holmes' deductions, reflecting the racial knowledge and beliefs of his times. For example, he shared contemporary beliefs in so-called "racial memory." After listening to the music of Norman Neruda he remarked to Watson: "It was magnificent. Do you remember what Darwin says about music? He claims that the power of producing and appreciating it existed among the human race long before the power of speech was arrived at. Perhaps that is why we are so subtly influenced by it. There are vague memories in our souls of those misty centuries when the world was in its childhood" (p. 29).

Holmes knew of the pygmoid people of the Malay Peninsula and of the Andamans. He used this knowledge toward the solution of *The Sign of the Four* (pp. 136–7). The race type most frequently referred to by him was the South European (Mediterranean). In *The Greek Interpreter* he and Watson were joined by a man "whose olive face and coal-black hair proclaimed his Southern origin" (p. 505). In *The Naval Treaty* Percy Phelps' fiancée is described as "with a beautiful olive complexion, large, dark, Italian eyes, and a wealth of deep black hair" (p. 519). In *The Adventure of the Six Napoleons* Inspector Lestrade refers to a dead man, saying that he had "some Catholic emblem round his neck, and that, along with his colour, made me think he was from the South" (p. 689). In *The Valley of Fear* Bodymaster McGinty is described as having a complexion "as swarthy as that of an Italian" (p. 971).

The Yellow Face is based on a race-mixture assumption which today we would hold utterly untenable. When the mask was removed "there was a little coal-black negress . . ." (p. 414). Mrs. Grant Munro, a white Englishwoman, explained by saying the child was hers, from a union with John Hebron, of Atlanta, Georgia. She showed his picture to Holmes; he was "strikingly handsome and

intelligent-looking, but bearing unmistakable signs upon his features of his African descent" (p. 414). This description certainly does not suggest a deeply-pigmented Negro face—there are only "signs upon his features" (possibly lips or nose?) of "African descent." The whole case is reared upon the hiding of the "coal-black" daughter of this marriage. As the evidence is laid before us it is genetically very improbable—if not impossible—that such a child could have resulted from the union of a white female and an apparently partially Negroid male. This case is built on the long-enduring myth of a total racial "throwback." In *The Adventure of Wisteria Lodge* reference is made to a "huge and hideous mulatto, with yellowish features of a pronounced negroid type" (p. 1034). Since *mulatto* generally refers to half-and-half it seems that negroid features were dominant.

Racial stereotypes and racial traits of personality abound in Holmes' cases. In *The Stock-Broker's Clerk* a Semitic stereotype is manifest. Reference is made to a "dark-haired, dark-eyed, black-bearded man, with a touch of the sheeny about his nose" (p. 419). In *The Naval Treaty* Watson said of Holmes, "He had, when he so willed, the utter immobility of countenance of a red Indian . . ." (p. 532). The Celtic type is a favorite. François de Villard is said by Holmes to possess "the Celtic power of quick intuition" (p. 93). In *The Musgrave Ritual* the jilted maid, Rachel, who murdered Brunton, is discussed by Holmes as follows: "what smouldering fire of vengeance had suddenly sprung into flame in this passionate Celtic woman's soul . . . ?" (p. 456). It is interesting that earlier Musgrave spoke of her "excitable Welsh temperament" (p. 447). Also at an earlier time, Holmes himself had mentioned to Watson Rachel's "Welsh blood, fiery and passionate" (p. 451). In *The Hound of the Baskervilles* Dr. Mortimer discusses and contrasts Sir Charles and Sir Henry. Looking at the latter he observes, "a glance at our friend here reveals the rounded head of the Celt, which carries inside it the Celtic enthusiasm and power of attachment. Poor Sir Charles' head was of a very rare type, half Gaelic, half Ivernian, in its characteristics" (p. 818). In *The Crooked Man* Watson speaks of Holmes' "red-Indian composure which had made so many regard him as a machine rather than a man" (p. 475). In *The Hound of the Baskervilles* Mrs. Stapleton is described by Holmes as "a woman of Spanish blood [who] does not condone such an injury so lightly" (p. 899). In *The Adventure of the Three Gables* Isadora Klein is described as "tall, queenly, a perfect figure, a lovely mask-like face, with two wonderful Spanish eyes" (p. 1216). In *The Adventure of the Sussex Vampire* Mrs. Ferguson is defined as "jealous with all the strength of her fiery tropical love" (p. 1223). This is an example of the stereotype of the heightened emotions attributable to a torrid climate. In *The Problem of Thor Bridge* Miss Gibson was described as "a creature of the tropics, a Brazilian by birth" (p. 1246). Miss Dunbar said of her, "She hated me with all the fervour of her tropical nature" (p. 1256).

Body build and behavior. Just as racial concepts and ideology reflected the beliefs of the day, so do interpretations of behavior based on bodily traits. There can be no doubt that Holmes leaned heavily upon phrenology and physiognomic patterns. There is also some evidence that the over-all body build was a factor in deducing possible behavioral traits. The word *constitution* is used frequently, but usually in a purely structural or habitus sense; there is little implication of constitutional "type." Yet Watson's description of Holmes strongly suggests definition of the linear type (asthenic, leptosome, ectomorphic): "In height he was rather over six feet, and so excessively lean that he seemed to be considerably taller. His eyes were sharp and piercing . . . and his thin, hawk-like nose gave his whole expression an air of alertness and decision. His chin, too, had the prominence and squareness which mark the man of determination. His hands [had an] extraordinary delicacy of touch . . ." (p. 9). In *The Naval Treaty* Holmes is described as drawing up his knees, as he sat, and closing his fingers around his "long, thin shins" (p. 517). *Constitution* is used in contexts such as the following: "My constitution has not got over the Afghan campaign yet" (p. 91); "I speak . . . as a medical man to one for whose constitution he is to some extent answerable" (p. 92); ". . . his constitution has been shattered by his life abroad . . ." (p. 236); "Even his [Holmes'] iron constitution, however, had broken down under the strain . . ." (p. 458); again, in 1897, "Holmes' iron constitution showed symptoms of giving way . . ." (p. 1124). It is clear, from these quotations, that the term is used synonymously with bodily health or well-being.

There can be no doubt that Holmes equated head size, brain size, and a high or vaulted forehead with intelligence and/or unusual mental ability. In *The Man With the Twisted Lip* Watson asked Holmes how he deduced that a man was intellectual. Holmes replied, as he fit a very large hat, belonging to the man in question, well over his own brow (it settled upon the bridge of his nose), "It is a matter of cubic capacity . . . a man with so large a brow must have something in it" (p. 279).

There is a recurrent phrenologic theme in Holmes' thinking. In *The Musgrave Ritual,* Brun-

ton, the very able butler who "speaks several languages" is described as having "a splendid forehead" (p. 447). In *The Final Problem* the arch-criminal Professor Moriarty possessed a "forehead [which] domes out in white curve" (p. 545). Upon appraising Holmes, a most respected antagonist, the Professor observed, "You have less frontal development than I should have expected" (p. 545). In *The Adventure of the Empty House* Col. Moran, the Professor's right-hand man, is possessed of "the brow of a philosopher" (p. 570). In *The Adventure of the Priory School* Thorneycroft Huxtable, M.A., is described as having a "well-shaped head" (p. 628). In the same case the Duke of Holdernesse is depicted with "a high forehead" (p. 650). In *The Adventure of Charles Augustus Milverton* that gentleman is said to have had "a large, intellectual forehead" (p. 669). In *The Valley of Fear* Inspector Alec MacDonald is described as follows: "a great cranium and deep-set, lustrous eyes spoke . . . of the keen intelligence which twinkled out from beneath the bushy eyebrows" (p. 908). In *The Valley of Fear* Brother Morris is given "a kindly face and a good brow" (p. 983). In *The Adventure of the Bruce-Partington Plans* we meet Holmes' brother, Mycroft. Over a rather massive body was "perched a head so masterful in its brow, so alert in its steel-gray, deep-set eyes, so firm in its lips . . ." (p. 1077). In *The Adventure of the Dying Detective* the murderer, Mr. Culverton Smith, a methodical and able man, had a "high bald head" and "a skull of enormous capacity" (p. 1102). Every one of the foregoing—be he able scholar or be he master criminal—had a large, frontal-domed, high-vaulted head.

On two occasions the reverse was true. Persons of less ability were depicted as literal low-brows. In *A Study in Scarlet* Enoch J. Drebber had a "low forehead, blunt nose, and prognathous jaw" (p. 20). In *The Hound of the Baskervilles* Selden, the escaped criminal, had "a beetling forehead" (p. 873).

Holmes did not depend upon the forehead alone. In *A Scandal in Bohemia* Count Von Kramm concealed the upper half of his face with a mask. "From the lower part of the face he appeared to be a man of strong character, with a thick, hanging lip, and a long straight chin suggestive of resolution pushed to the length of obstinacy" (p. 181).

The shape and the size of the nose seem to have been important in deducing temperament. Holmes himself had a "hawk-like nose" (p. 9). In *The Adventure of the Second Stain,* Lord Bellinger, twice Premier, was "high-nosed" (p. 761). In *The Hound of the Baskervilles* Dr. Mortimer had "a long nose like a beak" (p. 786). In *The Adventure of the Devil's Foot* Dr. Leon Sternberg, the great lion hunter and explorer, was depicted with "a hawk-like nose" (p. 1131). In *The Adventure of the Blanched Soldier* Col. Emsworth possessed "a red-veined nose [which] jutted out like a vulture's beak" (p. 1181). In *The Adventure of the Mazarin Stone* Count Sylvius had "a long curved nose like the beak of an eagle" (p. 1197).

Holmes approached a constitutional-type analysis in *A Study in Scarlet* when he deduced that the murderer must have had "a florid face" (p. 23) or was "ruddy-faced" (p. 86). The murder victim had not bled, yet there was blood at the scene of the crime; ergo, the murderer must have burst a vessel in the excitement. It may be conjectured that Holmes was familiar with the "Habitus Apoplecticus" of Hippocrates.

Holmes really went overboard in observing comparative behavior in *The Adventure of the Creeping Man.* Indeed he remarked to Watson, "I have serious thoughts of writing a small monograph on the uses of dogs in the work of the detective" (p. 1262). He went on to say, "A dog reflects the family life. Whoever saw a frisky dog in a gloomy family, or a sad dog in happy one? Snarling people have snarling dogs, dangerous people have dangerous dogs. And their passing moods may reflect the passing moods of others" (p. 1262).

In *The Naval Treaty* Holmes deduced personality from hand-writing. The letter from Ralph Phelps was, he said, in a woman's hand, "a woman of rare character" (p. 518). He went on speaking to Watson: "You see, at the commencement of an investigation it is something to know that your client is in close contact with someone who, for good or evil, has an exceptional nature." In this case the influence was for good—Annie, the sister of Joseph Harrison.

Holmes was an astute student and observer of human nature. His approach was most often that of the "clinician"—the experienced evaluator of the subjective, the apparently irrelevant detail, the total picture—rather than that of the highly trained scientific specialist. I suppose we may invoke the old cliché and say to him the personal factor was art rather than science.

Heredity. Holmes was convinced that heredity was not only organic, but behavioral as well. Time and again he made deductions or inferences on the basis of transmitted traits, usually along family lines.

In *The Adventure of the Empty House* Holmes discussed Col. Moran, a man of undoubted ability who had turned to crime. "There are some trees, Watson, which grow to a certain height, and then suddenly develop some unsightly eccentricity. You

will see it often in humans. I have a theory that the individual represents in his development the whole procession of his ancestors, and that such a sudden turn to good or evil stands for some strong influence which came into the line of his pedigree. The person becomes, as it were, the epitome of the history of his own family" (p. 573). In *The Adventure of the Copper Beeches* Holmes discusses Mrs. Rucastle's child, who alternates "between savage fits of passion and gloomy intervals of sulking" (p. 371). From observing this child, Holmes told Watson that he learned something about the parents. "I have," he said, "frequently gained my first real insight into the character of parents by studying their children" (p. 377). Holmes felt in this case that the child's abnormal cruelty derived from "his smiling father."

In *The Hound of the Baskervilles* a different approach was used. He covered part of the portrait of Hugo Baskerville so that facial features were emphasized. At once Watson observed that "the face of Stapleton had sprung out of the canvas" (p. 879). Holmes remarked that "it is an interesting instance of a throwback, which appears to be both physical and spiritual. A study of family portraits is enough to convert a man to the doctrine of reincarnation" (p. 879).

The hereditary transmission of more specific trait-patterns was accepted by Holmes. In *The Final Problem* he said of Professor Moriarty, "a criminal strain ran in his blood . . . rendered infinitely more dangerous by his extraordinary mental powers" (p. 544). In *The Adventure of the Speckled Band* Helen Stoner says, "violence of temper approaching to mania has been hereditary in the men of my family . . ." (p. 295). In *The Reigate Puzzle* Holmes discusses his deductions made from writing in the case: "I am sure that you cannot fail to be delighted with the traces of heredity shown in the *p*'s and in the tails of the *g*'s. The absence of the *i*-dots in the old man's writing is also most characteristic" (p. 473). In *The Greek Interpreter* Holmes and Watson discussed "how far any singular gift in an individual was due to his ancestry and how far to his own early training" (p. 502). Holmes felt that his own special skills were "in my veins and may have come with my grandmother, who was the sister of Vernet, the French artist. Art in the blood is liable to take the strangest forms" (p. 502).

The only morphologic trait discussed in hereditary terms is the ear. In *The Adventures of the Cardboard Box* Holmes said of Miss S. Cushing, "I was curious . . . to see how far the family ear had been reproduced in her" (p. 1054).

Holmes' concern with family-line inheritance is in keeping with his emphasis upon the observation of external features. To him, apparently, the English tradition of "family" was a certain guarantee of the long retention of both physical and behavioral patterns.

Miscellaneous: ear, teeth, fingerprints, hair, apes, aging. Over and beyond the more general data above presented must be listed certain more specific evidence of Holmes' knowledge of anthropology. In *The Adventure of the Cardboard Box* he said, "As a medical man, you are aware, Watson, that there is no part which varies so much as the human ear. Each ear is as a rule quite distinctive and differs from all other ones. In last year's *Anthropological Journal* you will find two short monographs from my pen upon the subject" (p. 1053). In *The Yellow Face* Holmes deduced from a pipe-stem that the owner had "an excellent set of teeth," because the amber was bitten through (pp. 403–4). Curiously enough, fingerprints figure prominently in only one case, *The Adventure of the Norwood Builder.* Oldacre tried to frame MacFarlane by putting his thumbprint on a whitewashed wall (p. 587). Lestrade had already printed the suspect's fingers, so a comparative check was made at once. It is a definite chink in Holmes' professional armor that he so neglected fingerprints. This is to be wondered at, for it was his own countryman, Galton, who figured prominently in developing the science of dermatoglyphics. Further, Bertillon had taken up fingerprint analysis. Holmes knew well of him, for in *The Naval Treaty* Watson referred to the "Bertillon system of measurements" and Holmes expressed his "enthusiastic admiration of the French savant" (p. 532). Hair does not figure prominently. In *The Crooked Man* Holmes complains that the animal (later identified as a mongoose) "has not been considerate enough to leave any of its hair behind it" (p. 480). From this we infer that if hair had been left behind Holmes could have used it in deducing the animal represented by the foot-prints. In *The Adventure of Shoscombe Old Place* Holmes was bending over a low-power microscope when he asked Watson to look in the eyepiece. As he did so Holmes said, "Those hairs are threads from a tweed coat . . ." (p. 1299).

Holmes had only one case which directly involved the nonhuman primates, *The Adventure of the Creeping Man.* In this case Professor Presbury was involved; for some strange reason he began crawling on all fours; more than that, he crawled up to his chained dog and teased it unmercifully. In one encounter with Holmes he "grinned and gibbered . . . in his senseless rage" (p. 1269). It was finally revealed that H. Lowenstein of Austria

was selling the Professor a serum to rejuvenate him, since the Professor was planning to marry a young lady many years his junior. In a letter to Holmes, Lowenstein explained the Professor's reactions as follows: "It is possible that the serum of anthropoid would have been better. I have, as I explained to you, used black-faced langur because a specimen was accessible. Langur is, of course, a crawler and climber, while anthropoid walks erect and is in all ways nearer" (p. 1275). The whole case is reared upon an untenable hypothesis—that is, langur serum would profoundly modify the Professor's behavior and even body physiology! Holmes said to Mr. Bennett that, "the dog, of course, was aware of the change far more quickly than you. His smell would insure that. It was the monkey, not the professor, whom Roy attacked, just as it was the monkey who teased Roy" (p. 1276).

In *The Adventure of The Speckled Band* there is another reference to a nonhuman primate, but it does not figure directly. Helen Stoner, in speaking of her stepfather, said, "He has a passion also for Indian animals, which are sent over to him by a correspondent, and he has at this moment a cheetah and a baboon . . ." (p. 295).

Holmes' knowledge of the nonhuman primates was woefully inadequate. It is true that an anthropoid is semierect on occasion, but its position is basically quadrupedal, not "erect," as Lowenstein said. The baboon, to which Miss Stoner referred, is indigenous to Africa and Arabia, not to India. It may have been, in Holmes' time, that macaques were mistakenly called baboons, but Holmes should have caught this error.

Holmes' knowledge of bodily manifestations of age was of considerable use in his case analysis. In *The Sign of the Four* he disguised himself as an aged man: "His back was bowed, his knees shaky, and his breathing was painfully asthmatic" (p. 142). In *A Study in Scarlet* he deduced vigorous middle age when he observed, "Well, if a man can stride four and a half feet without the smallest effort, he can't be quite in the sere and yellow" (p. 24). In *The Adventure of the Beryl Coronet,* a reference is made to Mary, the niece of Mr. Holder. Mr. Holder told Holmes that Mary did not go out much. When Holmes said, "That is unusual in a young girl," Mr. Holder replied, "She is of a quiet nature. Besides she is not so very young. She is four-and-twenty" (p. 351). Reference to such an age as "not very young" is probably more cultural than biological. An unmarried young lady of that age was verging upon the spinster category. In *The Reigate Puzzle* Holmes deduced age from handwriting. He spoke to Watson as follows: "You may not be aware that the deduction of a man's age from his writing is one which has been brought to considerable accuracy by experts. In normal cases one can place a man in his true decade with tolerable confidence. I say normal cases, because ill-health and physical weakness reproduce the signs of old age, even when the invalid is a youth. In this case, looking at the bold, strong hand of the one, and the rather broken-backed appearance of the other, which still retains its legibility although the t's have begun to lose their crossing, we can say that the one was a young man and the other was advanced in years without being positively decrepit" (p. 470).

Archeology

There is not much reference to archeology, and when it does occur it is only incidental. In *The Adventure of the Golden Pince-Nez* we hear of "the Addleton tragedy, and the singular contents of an ancient British barrow" (p. 709). In *The Hound of the Baskervilles* Dr. Mortimer is said to have been "excavating a barrow at Long Down . . ." (p. 836). In the same case we hear of Neolithic habitation sites on the moor (p. 829). In *The Adventure of the Devil's Foot* Holmes says to Watson, "Let us walk along the cliffs together and search for flint arrows" (p. 1130). Finally, in *The Adventure of the Three Garridebs,* we note that in the room of Nathan Garrideb "there was a cabinet of flint instruments. . . . Above was a line of plaster skulls, with such names as 'Neanderthal,' 'Heidelberg,' 'Cromagnon' printed beneath them" (p. 1235).

Ethnography

Ethnography figured quite prominently in many cases. Holmes' knowledge of primitive peoples and their cultures was quite extensive. In *The Sign of the Four* the poisoned dart and blowgun of the Andamanese was a critical factor in unraveling the case (pp. 136–7). Actually, the blowgun was not used by the Andamanese. Holmes was in error here, even though he did trace the racial origin of the dwarfed individual. In *The Adventure of the Sussex Vampire* the South American alkaloid poison, curare, is an important consideration (p. 1229). Holmes was one of the first detectives to encounter this deadly poison as a possible murder agent. In the same case he demonstrated a good general knowledge of vampirism. In his files, under "V" Holmes mentioned "Vampirism in Hungary" and "Vampirism in Transylvania" (p. 1219).

Australia figured quite prominently in several cases. In *The Boscombe Valley Mystery* the cry of "Cooee" is correctly identified as an Australian signal call (p. 239). Likewise, reference to the dy-

ing man's words "a rat" led to "Ballarat," a town in Australia (p. 240). It seems likely that Holmes had never been to Australia himself, but he did mention other travels. In the 3 years that elapsed between his "death" at the Falls of Reichenbach and his reappearance in *The Adventure of the Empty House,* he had, he told Watson, "travelled for two years in Tibet . . . visiting Lhassa, and spending some days with the head lama." He also told of passing through Persia, looking in at Mecca, and "a short but interesting visit to the Khalifa at Khartoum." After that he spent some months in France "in a research into the coal-tar derivatives . . . in a laboratory at Montpellier . . ." (p. 565).

In *The Adventure of the Devil's Foot* Holmes displayed a great interest in linguistics. Watson said of him, "The ancient Cornish language had also arrested his attention, and he had, I remember, conceived the idea that it was akin to the Chaldean, and had been largely derived from the Phoenecian traders in tin" (p. 1125). Later, as the case was successfully closed, Holmes said, "I think we may . . . go back with a clear conscience to the study of those Chaldean roots which are surely to be traced in the Cornish branch of the great Celtic speech" (p. 1142).

In *The Adventure of the Illustrious Client* Holmes instructed Watson to familiarize himself with Chinese pottery. In an intensive 1-day session at museums and with books Watson "learned of the hall-marks of the great artist-decorators, of the mystery of cyclical dates, the marks of the Hung-wu and the beauties of the Yung-lo, the writing of Tang-ying, and the glories of the primitive period of the Sung and the Yuan" (p. 1173).

In *The Musgrave Ritual* there is a passing reference to a *rite de passage.* The ritual itself, says Reginald Musgrave, "is a sort of ceremony peculiar to our family, which each Musgrave for centuries past has gone through on his coming of age . . ." (p. 449).

Thus we see that Holmes' knowledge of Ethnography was at least quite diversified: material culture (blowgun, curare, pottery); linguistics; and travel and geography.

Epilog

Today, with our modern techniques, our specialized knowledge, and our interdisciplinary integration, it is all too easy to sit in the seat of the scornful and say that Holmes' knowledge and use of anthropology was—to use his own term—"elementary." Certainly he made "boners" in osteology, his knowledge of the infrahuman primates was woefully scant, and his failure to use fingerprints in detection was a serious gap in his technical skills! These are, I feel, the inadequacies that make Holmes, the man, human—and not the omniscient superdetective Watson built him up to be. All in all, let us accept his use of anatomy (including "physical anthropology") as at a pretty high level. Mayhap his monograph on tobacco and tobacco use and his essay on bees were more objective and scientific, based, as they were, upon personal experimentation and study. Yet, I for one, am willing to recognize in Sherlock Holmes a most worthy colleague in anthropology—the science of man an his behavior!

Screech Owl with Left Pupil Larger than Right

The interesting difference in pupil size that can be seen on this month's cover was brought about by shining a flashlight into the right pupil of a young screech owl and then taking the picture with a flash that overcame the illumination of the flashlight. This shows the ability of owls to change the pupil size very rapidly, which is an accommodation for quick-changing light intensity during night flying.

Latent-Image Formation and Chemical Sensitization

W. F. BERG

W. F. Berg is in charge of emulsion research at Kodak Ltd., Harrow, England. Born in Göttingen in 1908, he received his university training in physics at Berlin and at Manchester, England. His research activities have ranged from x-ray investigations of crystal imperfections to the fluorescence of iodine vapor, and since 1936 they have been concentrated on the theory of the photographic latent image. The following article appeared in the London Times Science Review *and is reprinted here by permission.*

PHOTOGRAPHY has been practiced in its present form for over 70 years (Fig. 1), and its mechanism has attracted the attention of research workers at universities and in industry. In spite of all their efforts, the preparation of the light-sensitive medium, the photographic emulsion, is essentially an empirical matter and improvements have come as a result not of predictions by the "backroom boys" but of the bottle-from-the-shelf approach. Understanding of the process has invariably followed empirical findings; this, of course, is not meant to lessen the value of the research that has been done, which has led to considerable refinements and improved control over the manufacturing processes.

What, then, is the difficulty? Our films, plates, or papers are covered with a thin layer of a yellow or white paintlike substance. This is the light-sensitive emulsion, which is in reality an exceedingly fine suspension of microcrystals of silver halide (bromide, chloride, iodide, or a mixture) in gelatin. The number of crystals per unit area reaches astronomical proportions, being some 100 million per square inch. The crystals are on the limit of resolution of the optical microscope and yet they are large on the atomic scale. They are built of a regular cubic latticework of alternate positively charged silver and negatively charged halide ions, and the average crystal in a film contains again about 100 million pairs of these ions—silver atoms that have lost an electron, and halogen atoms that have gained one from a silver atom.

On exposure to light, these microcrystals darken, or "print-out" (Fig. 2, left and middle), as do many silver compounds. The light decomposes them into metallic silver, which is black because it is very finely dispersed, and into molecular halogen. Under suitable conditions, each quantum of light absorbed produces one atom of silver. If photography had to be based on the print-out process, it would never have achieved the importance it has today. If we can succeed in capturing events as fleeting as the flip of the tongue of a chameleon (Fig. 3), it is because of the discovery of the latent image: the fact that a very much briefer exposure to light is sufficient to register in the emulsion layer as a "latent," invisible image that can be revealed by a mild reducing solution, known as the developer.

The developer reduces to metallic silver the whole of the light-struck emulsion crystal—provided it is allowed to act for a sufficiently long time—developing at the same time only a small fraction of the unexposed "fog" crystals. Its use provides us with an enormous increase in the effective sensitivity of the photographic emulsion and also with a persistent headache when we try to understand its action. We possess no chemical or physical method of detecting a latent image other than that of development. The exposure required to produce a latent image cannot possibly affect more than 10 or, at most, 100 of the ion pairs in the emulsion crystal, and, in the process of detection, both the latent image and its parent crystal are lost. Furthermore, the crystals cannot, in practice, be studied in isolation: their behavior can be assessed only on a statistical basis. Our problem is therefore as difficult as that of a biologist if he had to study the problems of life by the investigation of dead matter, without being able to see the individual cells.

The study of the photographic latent image has, in fact, many of the fascinations of biology. It is a borderline subject, involving physics, chemistry, physical chemistry, and that special brand of science labeled by a well-known worker as "metachemistry." The behavior of matter at surfaces and

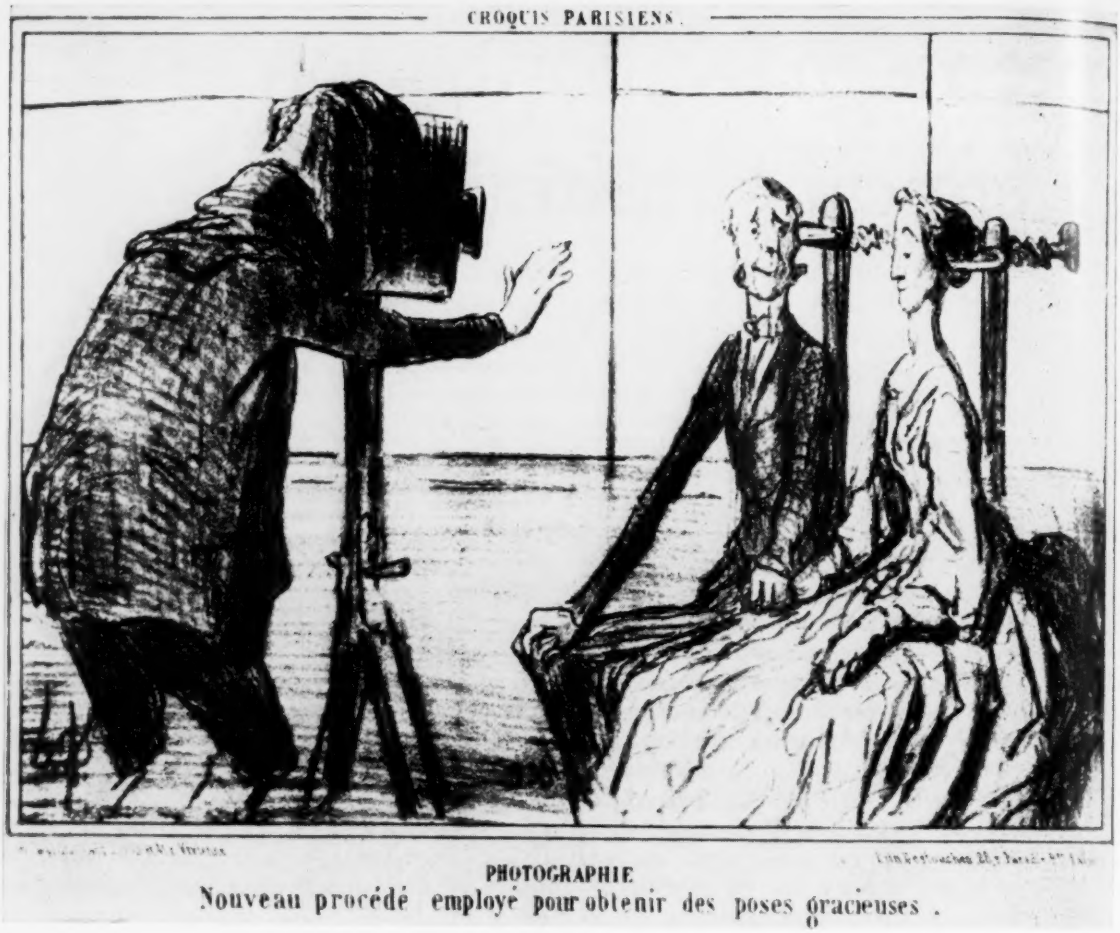

Fig. 1. Daumier's sketch of early photography.

interfaces is often quite different from that in bulk, and this opens wide the door to let in highly speculative thought by the more imaginative workers in the field.

Three fundamental questions arise: What is the nature of the latent image? How does it increase the rate of development of the exposed crystal? How is it formed? Clearly, a division such as this is artificial but necessary to organize one's thoughts.

Since continued exposure to light produces metallic silver, the most obvious assumption is that the latent image is merely a smaller amount of silver. This simple concept, however, met with great difficulties when the chemical behavior of the discoloration produced in silver halide crystals (by printing-out) was found to differ considerably from that of metallic silver: it was often very much less readily attacked by an oxidizing agent. The suggestion was made that the latent image might consist of an "oxyhaloid," an unspecified compound of silver, halogen, and oxygen; this was soon ruled out on purely chemical grounds. A more serious suggestion assumed that the latent image was a subhalide, a silver-halogen compound that contains less than the usual proportion of halogen, as for example, Ag_2Br. This compound could never be isolated, but the existence of the corresponding subfluoride made the suggestion plausible. In a wider sense, present-day concepts of the solid state do allow for subhalides: a silver bromide crystal may contain surplus silver in the form of F-centers, places in the lattice where a halide ion is missing, its place being taken by an electron. This electron, together with one of the silver ions in the neighborhood, constitutes an excess silver atom in the crystal. It is indeed now believed unlikely that silver halide crystals normally contain exactly equal numbers of both ions.

The subhalide concept accounted for the apparent inertness of the print-out silver in silver halides. It was assumed, very naturally, that any such silver would be situated on the surface of the crystals. We have since learned that a latent image can also be formed inside the crystals, where it is accessible to a developer only if the surface of the crystals is first dissolved away, and that we must dis-

tinguish between internal and superficial latent images. The internal latent image is protected against the attack of most oxidizing agents, whereas the superficial latent image reacts chemically, much like finely dispersed metallic silver.

For these reasons opposition faded, and the idea became generally accepted that a latent image is a minute speck of metallic silver in the surface of the emulsion crystal. This speck is thought to act as a catalyst that speeds up the reduction rate of the crystal by the developer solution, that offers, in the broadest general terms, electrons to the silver ions, thus turning them into silver atoms, and that takes up the halide ions, whose concentration in the solution increases as long as development continues.

The formation of the speck of silver is the most interesting puzzle in latent-image theory. From the fact that the spectral sensitivity of an emulsion conforms accurately to its absorption and from the total amount of light absorbed, we are forced to conclude that the whole of the crystal collaborates in the process. Light is absorbed all through the crystal, and yet the silver in the end appears at one or, at most, a few places. While we can be sure of this only for exposures that are sufficiently heavy to produce visible specks, a strong indication that the latent image is also in the form of localized specks lies in the finding that development always begins at a few isolated spots, "development centers" (Fig. 2, right), that enlarge and spread through the crystal as development continues. How then does the silver migrate through the crystal to coagulate at the site of the latent-image speck?

This question began to intrigue research workers some 30 years ago. They first wanted to know what it was that caused the silver to coagulate where it did. They had found that, in general, the sensitivity of emulsion crystals increased with their size. This was plausible, assuming that the silver coagulated, since the larger crystal would absorb more light. However, they also found that sensitivity varied considerably with crystals of the same size. There was clearly a factor connected with the preparation of the emulsion that caused a wide spread in sensitivity. In his classical work, Sheppard found a clue in the part played by certain sulfur compounds in increasing the sensitivity. These compounds occur naturally in the gelatins from certain animals, like cows, who have a taste for spicy herbs, such as mustard. Mustard oil, an organic compound containing sulfur, can be demonstrated to exist in gelatins and to cause a large increase in the sensitivity of an emulsion prepared in such a gelatin as compared with a control prepared in an inert gelatin from which all traces of sulfur compounds have been removed. As Mees once put it, "if cows did not like mustard, we could not go to the movies." We have since learned to make gelatins active by adding suitable sulfur compounds.

These sulfur compounds, which are characterized by a C=S double bond, are very strongly adsorbed to silver halide surfaces and, on suitable heat treatment, break down to silver sulfide in the form of localized specks. Since it is a similar gentle heat treatment in a solution of gelatin containing sulfur compounds that imparts sensitivity to an emulsion, Sheppard concluded that here, also, minute specks of silver sulfide were formed on the crystal surfaces. This conclusion is an extrapolation rather similar to that which led from the visible print-out silver speck to the assumption that the latent image was also a silver speck, although much smaller.

The silver sulfide "sensitivity speck" was thought to function as a nucleus on which the latent-image silver could coagulate, rather as a particle of soot

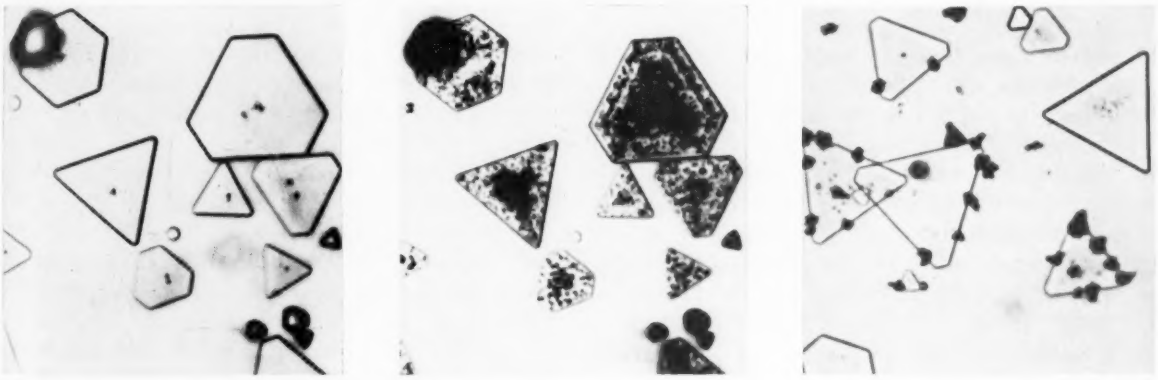

Fig. 2. (Left and middle) Photographic emulsion crystals at two stages of printing-out. The black spots are metallic silver, the pattern probably revealing places of physical imperfection. (Right) Emulsion crystals at an early stage of development, showing the development centers either in the center or along the edges. [Courtesy J. H. Tabor and Kodak Ltd.]

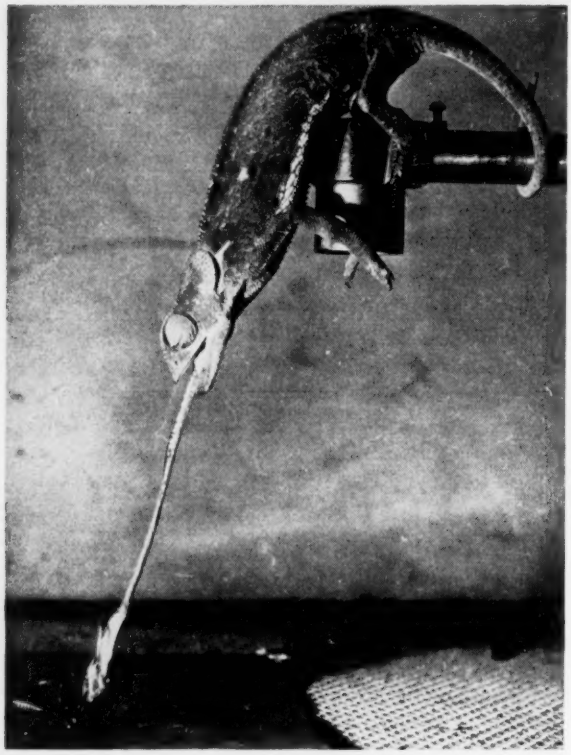

Fig. 3. Example of high-speed photography taken with Dawes Microflash Unit. [Courtesy London Zoological Gardens and Kodak Ltd.]

acts in promoting the precipitation of water to form fog. The concept of the "concentration speck" proved most fruitful, and it is the basis of latent-image theories to this day. Thus, it accounts for the preference of the latent image to form on the surface of the crystals rather than in their interior where concentration specks are less likely to be formed.

The concept of the concentration speck has inspired much research, which, while not necessarily explaining the fundamental mechanism, has reduced to some law and order the multitude of "photographic effects" that have now been largely rounded up and fitted into a few much simpler pictures. The basic idea was to regard latent-image formation as a condensation or precipitation phenomenon, not worrying about what was condensing. Condensation naturally occurs first on the most efficient nuclei. Hence, the latent image is formed first on the surface of the crystals. But if the latent image is produced at a high rate by a brief exposure at high intensity, many fine particles are formed: the latent image now also condenses on inefficient nuclei on the surface and in the interior of the crystals, and, since the particles are small, their catalytic effect is reduced and the rate of development is slowed down. Work along these lines is still in progress and has the advantage that its validity does not depend on the correctness or incorrectness of any assumptions for the fundamental process; furthermore, it provides a much simplified and sounder body of evidence for the theoretician to explain.

No satisfactory mechanism to account for the movement of the silver atoms through or along the surface of silver halide crystals was put forward until 1938. Then Gurney and Mott suggested their two-stage process of coagulation of silver. They pointed out that it was unnecessary to assume the movement of silver atoms through the body of the crystal, the silver atoms being too large to squeeze through the spaces between the ions. Instead they pointed to two well-known facts that together allowed the effective transport of atomic silver. Silver ions are mobile in the crystal, which can easily be demonstrated by placing a pair of electrodes on the crystal and applying an electric potential. The crystal then decomposes, the positively charged silver ions moving through the lattice, carrying an electric current that can be measured. A certain number of silver ions are, because of the local movement caused by temperature, thrown out of their normal positions in the crystal lattice into spaces where they will fit with a slight distortion of the surrounding lattice structure. These out-of-place silver ions can move about more easily the higher the temperature; at sufficiently low temperatures, they freeze in. This effect is known as ionic conductivity; it will take place in the dark (Fig. 4, top). If, however, the crystals are exposed to light, electrons are released by absorption of light and they can move freely through the crystal. This second effect is known as photoconductivity (Fig. 4, bottom). The electrons are more mobile than the silver ions.

Since a silver atom is an ion plus an electron, the movement of electrons and ions can equal a movement of silver atoms. Atoms will coagulate at a center if the electrons are caught there. Electron capture, on this picture, is therefore the function of the sensitivity speck. Gurney and Mott assumed that the electron would occupy a state of lower energy in the silver sulfide crystal than in the silver halide crystal, and that, therefore, silver sulfide specks would "trap" electrons. The sensitivity speck becomes negatively charged and pulls any loose silver ions toward it. When an ion reaches the speck, it combines with the electron and forms a silver atom. The speck is then able to trap another electron, so that the process repeats itself until the speck grows to a size sufficient to induce development (Fig. 5).

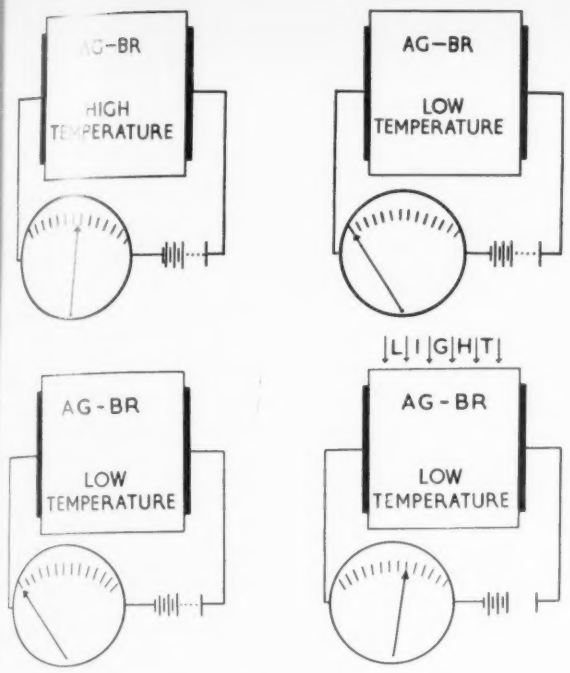

Fig. 4. Diagrammatic representations of ionic conductivity (top) and photoconductivity (bottom). [From *J. Phot. Science* **1**, 4 (1953)]

This mechanism provided a picture that for the first time held out hope of accounting for all our experiences. In particular, it explained the great changes in the sensitivity of photographic emulsions with temperature and occasioned an outburst in research activity, the result of which appeared to prove the consistency of the picture, even if direct proof of its basic assumption, the trapping of electrons by the sensitivity speck, was not possible.

The picture was not complete, however. It "tells us all about the hero, but nothing about the heroine," the hero here being the silver, the heroine the halogen. The electron whose release by light and movement through the crystal constitutes photoconduction originates from a halide ion, which, by absorption of light, is changed to a halogen atom. Silver and halogen atoms are thus counterparts. If they were to meet again, they would recombine to form silver halide, since there would be no more free silver, and no latent image would be formed. Clearly, our picture of the photographic process is incomplete unless we can account for the movement of the halogen and ensure that it is safely out of the way so that it cannot attack the latent image.

The role of the halogen atoms was overlooked for many years. In order to transport them through the crystal, physical movement of the atoms is not necessary. A halogen atom can move by one lattice distance if an electron from a neighboring halide ion moves over to the halogen atom: by this simple electron transfer, the atom and ion change places. Since only the movement of electrons is involved, a halogen atom is, in effect, highly mobile; it behaves like a positively charged electron and is therefore, in the jargon of solid-state physics, described as a defect electron, or "positive hole." A halogen atom would far outpace the relatively sluggish silver ion in a race toward the negatively charged sensitivity speck unless a mechanism existed that would take care of it in some other way.

Earlier pictures skated around the problem by suggesting, for example, that the halogen would combine with the gelatin surrounding the emulsion crystals or would be taken up by the water left in the emulsion layer. We now feel that this is not adequate and that it does not account for the facts of chemical sensitization.

In recent years, an increasing number of papers have been published on chemical sensitization, by which we mean the result of the treatment given to a photographic emulsion in order to increase its sensitivity to light other than by increasing the size of the sensitive crystals. Chemical sensitization was once part of the secret armory of the emulsion-maker, hidden behind a "silver curtain," since most of his methods could not be patented, and it formed the know-how on which any one manufacturer's success was based. All this has altered as

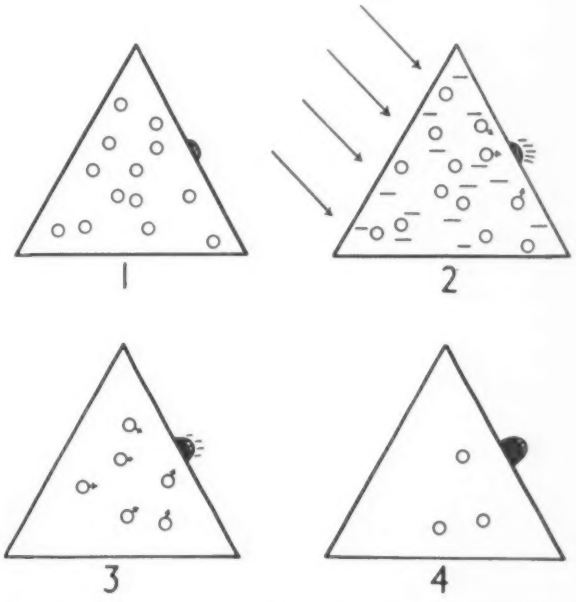

Fig. 5. The mechanism of latent-image formation after Gurney and Mott. The circles (o) represent mobile silver ions, the dashes (–) electrons, the hatched speck on the surface the sensitivity speck, and the solid black speck the latent image. [From *J. Phot. Science* **1**, 4 (1953)]

the result of the publication after World War II (*1*) of many of the manufacturing methods of the German factories. Many previously secret procedures are now general knowledge, and this has made it possible and profitable to subject chemical sensitization to closer academic study and to publish the results.

From a technologic point of view, the most important recent advance is said to have resulted from sensitization by noble metals, such as gold; a doubling of the sensitivity for the same average size of the emulsion crystals is claimed. Another technologically less important method that has only recently been clearly recognized utilizes certain reducing agents and cannot be imagined to produce anything but silver, although the chemical reactions of the result of the sensitizing treatment are not quite identical with those of the latent image. Yet another kind of sensitization is brought about by mere contact of silver halide with inert gelatin; although discovered some 60 years ago, it was not studied until recently. This again had not been recognized earlier, presumably because gelatin is an essential ingredient of the emulsion. Together with sulfur sensitizing, there are thus at least four distinct kinds of chemical sensitization.

Our knowledge is insufficient to enable us to understand the mechanism of the various methods of sensitization, but it is clear that the study of latent-image formation and chemical sensitization must go hand in hand. Thus, recent work in this field tends to regard these two aspects as a single problem.

In principle, there are three distinct ways in which a chemical sensitizer can improve the sensitivity of a silver halide crystal, although to add to our complications, it is quite possible that any one sensitizer might fulfill more than one of these functions. First, it could enable a small latent-image speck to induce development, where previously a larger speck was necessary. This, according to our best guess, is the function of sensitization by noble metals and perhaps sometimes by sulfur compounds. Second, it could form a more efficient nucleus for coagulation of the latent-image silver, thus producing a single large latent-image speck in place of a number of smaller ones. And third, the sensitizer might be an effective acceptor of halogen atoms; a clear field would thus be left for the silver atoms, and the photochemical efficiency of the photographic process would be genuinely increased.

An analysis of the current publications on chemical sensitization would take us too far and would probably be confusing. However, one idea keeps recurring with increasing frequency: that chemical sensitization is in the main concerned with halogen acceptance in a form that eliminates the production of free halogen, at least in the early stages of latent-image formation.

Reduction sensitizing is clearly a way of producing a silver halide crystal containing an over-all surplus of metallic silver. It is possible to regard the formation of silver sulfide in a similar light: it may be incorporated into the crystals in such a way as to represent effectively a surplus of silver. In these systems, then, exposure to light would, in its first stages, merely cause the surplus silver to be rearranged to produce the latent image; the surplus silver would constitute the most effective kind of halogen acceptor since it is present in or on the crystal where it is wanted most. Too great a surplus would produce silver specks akin to the latent image. The crystals would develop without exposure, constituting the chemical fog or veil only too well known by the emulsion-maker to result from oversensitizing the emulsion.

Attention has also been drawn to the light sensitivity of the surface complex of gelatin, silver ions, and hydroxyl ions; a latent image is formed in this, again without the release of halogen. In a medium with a large surface per unit mass, such as an emulsion, the behavior of the surfaces is likely to prove to be the key to the problem.

Apart from the chemical sensitizers, the surface of an emulsion crystal is densely populated by numerous chemicals introduced to improve the emulsion. The most important class is the sensitizing dyes that extend the spectral region of absorption and sensitivity of the emulsion to longer wavelengths. Recent work has revealed a lively competition between all these agents for the crystal sur-

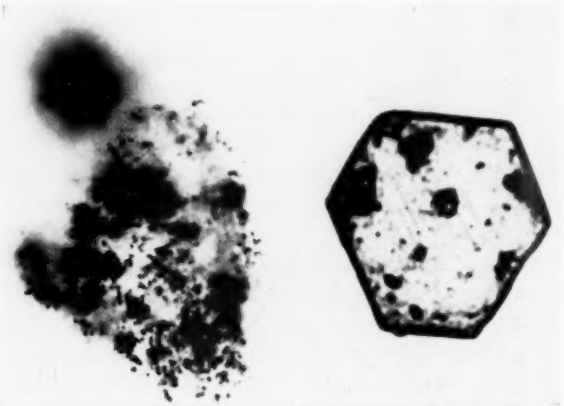

Fig. 6. (Left) Development centers on the surface of a grain of silica sensitized with silver and hydroxyl ions. (Right) A photographic emulsion crystal. [Courtesy E. E. Loening and Kodak Ltd.].

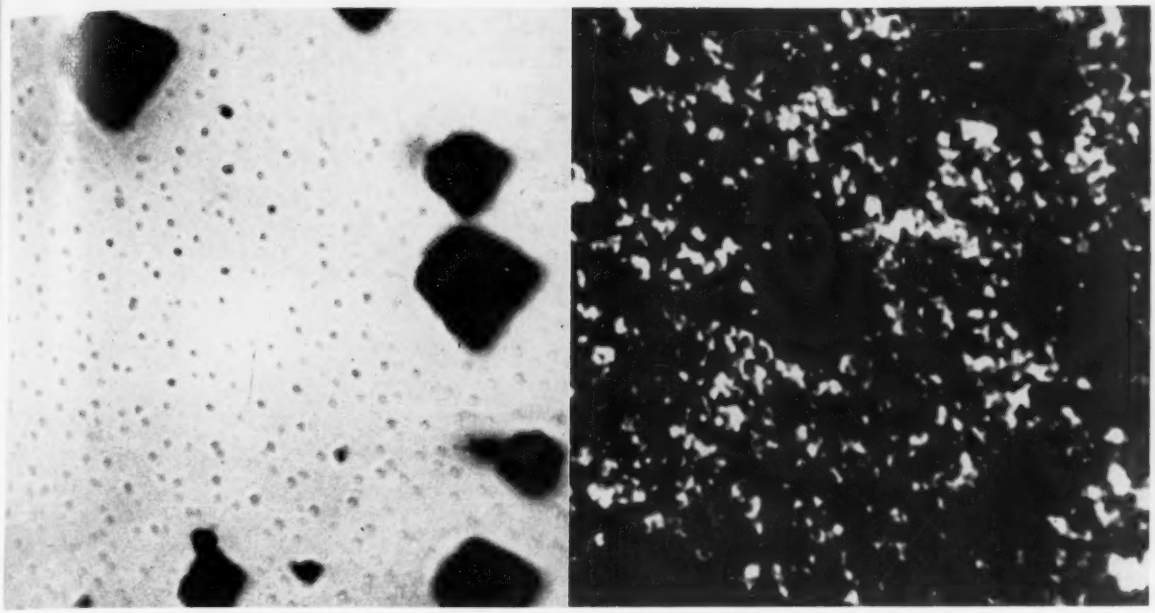

Fig. 7. (Left) Photomicrograph of the surface of a silver bromide crystal prepared from the melt, sensitized by evaporating a layer of gold on it, and developed. The top half is unexposed; the big black specks are developed "fog" silver. (Right) Print-out silver deposited in the gaps between neighboring perfect regions of silver bromide crystals prepared from the melt and then annealed. [Courtesy J. M. Hedges and J. W. Mitchell, University of Bristol, England.]

faces, almost like plants fighting for a place in the sun.

It has become clear that the complete photographic emulsion is too complicated a system for research and that a fresh experimental approach is wanted. The emulsion must be taken to pieces, the pieces studied separately, and then put together again for further study if we are ever to understand its behavior in the detail now desired. Thus, silver halide crystals have been investigated in the form of a sol, that is, a fine, very dilute suspension without a protective colloid, such as gelatin. The behavior of silver ions adsorbed to chemically inactive surfaces, such as silica (Fig. 6), has been found to be strikingly similar to that of photographic emulsions: in particular, surface complexes of silver with hydroxyl ions increase the sensitivity in both systems. This and the silver-ion-gelatin combination are light-sensitive systems of which we know little; the mechanism for the formation of a latent image in them is likely to be an involved and intriguing problem.

Another new experimental method is the study, by emulsion standards, of very large crystals of silver bromide that have been grown from the melt. Suitable heat treatment of such crystals produces specimens that behave in many respects like a photographic emulsion and that may be chemically sensitized by all the known methods (Fig. 7, left). They have provided the most striking demonstration of the free mobility of bromine atoms and of that kind of chemical sensitization that is concerned with halogen acceptance: without a suitable sensitizer, such crystals do not produce a superficial latent image. One of the most important findings from the work on single crystals was the part played by physical imperfections of the structure. The perfect crystal appears to be practically insensitive to light; imperfections are necessary to allow a latent image to be formed. Considerable importance is attached to the higher chemical reactivity of the imperfect regions of the crystal.

In conclusion, reference must be made to one of the most thrilling findings in this single-crystal work that is connected not so much with photography as with the solid state. For years, in order to account for the low physical strength of certain crystals, the theoretical physicist has used the picture of the dislocation, a misfit of two neighboring planes of a single crystal, whereby one plane contains one more row of atoms or ions than the next one. Heat treatment causes these dislocations to run toward the crystal boundaries, leaving the crystal in more perfect condition and the boundaries rather more heavily disturbed; a single crystal containing dislocations will subdivide to form a substructure, the fragments of which are slightly misaligned with respect to one another.

Exposing a crystal to light causes print-out silver to be deposited in the resulting gaps between the perfect regions, thus providing very beautiful and direct evidence for the dislocation picture (Fig. 7, right). Here we have a striking instance of research in one field giving most rewarding results in another.

If we have to leave our problem with a large question mark, it is not without hope that a more coherent and simpler account may perhaps be around the corner. The suggestions mentioned are to be regarded as working hypotheses rather than as mechanisms to be placed as accepted in students' textbooks, and they appear to fulfill their functions well, judging by the intensity of research work at present in progress.

Reference

1. In BIOS (British Intelligence Objective Sub-Committee) and FIAT [Field Information Agency (Technical)] reports.

The Prisoner

Searching, seeking, plumbing down
That luring, labyrinthian well,
You cast aside your cap and gown
And rake afire the coals of hell.
For knowledge of that awful place
And mingled scenes of fairer stance
Comprise the gamut you embrace
And limit you with circumstance.

Fettered, blinded, hypnotized
By wonders deep and vague and real,
You seek an answer, cauterized,
And tamper with Pandora's seal.
Within that whirling mass of light
Your mind is pressed to overflow
To find the Truth in such a night
When all you know is what you know.

Thinking, writing, speaking out,
With words metallic at their best,
You clamor, scoffing in your shout,
There is no home or place to rest
Except this all engulfing wave
Which washes not on foreign land,
And washing not, from out its cave
Denies the pebbles in the sand.

GEORGE K. ESTOK

Identification and Guidance of Gifted Children

ANTONIA BELL MORGAN

Mrs. Morgan, a psychologist with Aptitude Associates Incorporated in Merrifield, Virginia, received her training at Somerville College, University of Oxford and at the University of London, where she specialized in educational psychology. In 1941–42 she traveled in the United States as Walter Hines Page scholar, and lectured on British educational problems in wartime. On her return to England she worked for the Ministry of Information and later served as assistant principal in the India Office, where she conducted research studies on the social and educational problems of India. She is now a U.S. citizen and is engaged in clinical and research studies of gifted children.

THE purpose of this paper (*1*) is to point out the need for more effective measures for the identification and guidance of gifted children at early ages in the public elementary schools, and to outline a tentative plan to meet this need.

It is axiomatic that gifted children are a valuable national asset—possibly the most valuable one. In the words of Agnes Meyer, they form "a huge reservoir of human talents, capacities and skills" (*2*).

The first systematic plan for the identification and guidance of gifted children was put forward 2350 years ago by Plato. He proposed that the leaders of the nation be selected by a series of tests, both *academic* and *situational*, beginning in early childhood (3, 413C ff.). Only children who passed these tests were to be admitted to the higher studies of mathematics and dialectics. He wrote (*3*, 413D–E):

> We must watch them from their earliest childhood. We must select those who are tenacious of memory and hard to deceive. The others we must reject. . . . We must impose upon them, too, labors and vexations and contests . . . testing them more rigorously than gold is tested in the fire.

The children who performed well on these tests were to be chosen for higher education. Plato was far ahead of his time in insisting that exactly the same education and the same tests be given to both sexes, although he considered that the average performance of girls was certain to be inferior in all respects to that of boys (*3*, 451D ff.). Unfortunately Plato's proposals were never put into effect.

In the succeeding centuries gifted children were identified chiefly on the basis of *academic achievement*, which taken alone is an unreliable index of talent. The younger William Pitt, for example, was something of a prodigy, but Winston Churchill was considered a slow learner in school. *Situation tests*, of a certain kind, have long been in use in the English public schools, in the guise of sports. School sports, in English schools, are essentially a series of situation tests. Every pupil has to take part in them, and the object is not to entertain but to reveal and foster qualities of leadership. The Duke of Wellington is purported to have said that the Battle of Waterloo was won on the playing fields of Eton. Belief in the value of this type of situation test has become firmly entrenched in the English public and preparatory schools, largely because of the work of Thomas Arnold of Rugby in the early 19th century. In the United States, so far as I know, no large-scale attempt has been made to introduce situation tests to identify leadership potential in the early years of life. There is scope for investigation along these lines.

In scientific research on gifted children during the last 30 years the United States has been outstanding. Foremost, of course, is the great contribution made by Terman and his associates both in the standardization of the Binet mental tests (*4*, *5*) and in the follow-up studies of 1500 children who scored high on these tests (*6–8*). Others have added to our knowledge in this field, but there is still a wide gap between this scientific knowledge and its application. Many gifted children are not identified at all, or identified too late, or identified but not given effective guidance.

Geographic Distribution

Evidence that identification and guidance of the gifted are inadequate may be gained from an

examination of statistics on the winners in the annual national Science Talent Search for the Westinghouse Science scholarships for the 13 years in which the search has been in operation (*9*). The Science Talent Search (STS) is open to all secondary-school seniors, and each year 40 winners are chosen; thus in the last 13 years there have been altogether 520 winners. This means that 520 gifted young people have received effective guidance toward scientific achievement. Guidance, of course, presupposes identification. If effective measures for the identification and guidance of the gifted were evenly distributed throughout the nation, we should expect that the number of winners from each state would be proportionate to that state's population. This is not, in fact, the case.

In 18 states the difference between the number of winners expected on a population basis and the actual number of winners exceeds 100 percent (*10*). This list excludes states with a population of less than 1 million, where chance factors play too large a part in the calculations. Two of the 18 states, New York and Oregon, have more than double the number of winners expected. For New York the expected number of winners is 43, the actual number 190. For Oregon the expected number is 6, the actual number 13.

In the other 16 states, the number of winners is less than half of expectation. These states are Alabama, Arkansas, North Carolina, South Carolina, Florida, Georgia, Iowa, Kentucky, Louisiana, Maryland, Michigan, Mississippi, Tennessee, Texas, Virginia, and Washington. Michigan, for instance, has three winners, with 20 expected. It might be thought that in these 16 states there has been less active interest in Science Talent Searches, but this is not true. Nine of these 16 states hold annual state science talent searches of their own, based on the entries in the national STS (*11*). Neither New York nor Oregon, states that exceed expectation by more than 100 percent, hold STS's of their own. The low proportion of winners cannot, therefore, be caused by lack of interest in the STS.

A detailed examination of the lists of winners and honorable mentions (260 per year) in the national STS will reveal differences among areas within a state. For example, in New York, 50 of the 190 national winners, or 28 percent, came from four high schools in the City of New York area (*12*). Again, in Virginia, of the 26 honorable mentions obtained during the last 5 years, nine, more than one-third, came from a single high school (*13*).

These figures show that measures to identify and guide gifted children are very unevenly distributed. For instance, a gifted child living in New York is 29 times as likely to become a winner in the national STS as a gifted child living in Michigan. What are some of these measures, and on what assumptions are they based?

Present Status of Gifted Children

The great majority of special provisions for gifted children are to be found at the high-school level. In high schools, potential tends to be equated with achievement. Some gifted students who have failed to achieve success in school may escape identification altogether. But even those who are identified as gifted in high school may have been identified too late because, through lack of guidance, they failed to use the earlier years to the best advantage. It is widely held that, to be most effective, indentification should take place and guidance begin, at the latest, in the elementary school (*14*, p. 16).

What is the status of gifted children in the elementary school? Broadly speaking, there are three methods of dealing with them: homogeneous grouping, enrichment, and acceleration.

Homogeneous grouping. Homogeneous grouping is widely regarded as the best solution. Its value was demonstrated by Leta Hollingworth's classic experiments in the city schools of New York (*15*). Homogeneous grouping has been recommended by the Educational Policies Commission of the National Education Association (*16*). Its advantages have been ably set forth by Hildreth, Brumbaugh, and Wilson in their report of the work of the Hunter College Elementary School for Gifted Children (*14, 17*). The Hunter College school selects its pupils on the basis of an individual intelligence examination, the Stanford-Binet, administered to all applicants for admission. Its pupils range in age from 3 to 11 years. The advantage of this system of selection is that it provides incentive for early identification. A limitation of the system is that it misses the children whose abilities have not already been discerned by others. *It happens more often than is commonly supposed that parents and teachers fail to realize that a child is gifted.*

Another system of homogeneous grouping is used in the Astoria and Corvallis districts in Oregon (*18, 19*). Pupils in the fourth and fifth grades are screened by group intelligence tests, school achievement tests, and teachers' ratings. Pupils who score high are tested individually; and, on the basis of the individual tests and conferences between parents and school officials, certain children are grouped together in a special class. This is a pilot program, and it is too early to comment on its

results. The system of selection is, however, open to two objections: (i) it delays identification until the fourth or fifth grade; and (ii) children selected must be not only intellectually superior but educationally advanced. Here, as in high schools, *potential* is defined in terms of *achievement*.

New York and Oregon are not, of course, the only states in which there are special classes for gifted children in the elementary schools. Such classes are found, for example, in Cleveland (*20*) and Los Angeles (*21*). A different approach to homogeneous grouping is found in the Havighurst Community Project in Chicago where the resources of the entire community are mobilized to meet the needs of all exceptional children (*22*). It is doubtful, however, whether this particular approach would be as successful in communities with a larger proportion of transients and a greater variation in educational level.

The advantages of homogeneous grouping of gifted children have been amply demonstrated. But for a great number of school systems this is out of the question. Homogeneous grouping is possible only in heavily populated areas. In areas of more scattered population the problem of providing transportation is too great. Except in large cities and their environs, therefore, the needs of gifted children must be met by either enrichment or acceleration.

Enrichment. The term *enrichment* is used here to mean the provision of additional work for gifted pupils by the regular teacher in the regular classroom. This is the sense in which the term is most widely used. Certain school systems, however, in which there are special classes for the gifted, refer to the program of study offered to these special classes as "enrichment of the curriculum" (*23, 24*). This has led to confusion because *enrichment* in the latter sense is one aspect of homogeneous grouping, not an alternative to it.

Theoretical considerations in regard to enrichment have been discussed by Hildreth, Brumbaugh, and Wilson (*14*, p. 58). Here it is enough to point out the practical difficulties of enrichment at the elementary level. In a high-school class, even a large one, it may be possible to carry out a useful enrichment program for certain subjects, as has been shown by Jewett (*25*). But, as Witty and Bloom have pointed out (*26*), in an elementary-school class of from 30 to 40 pupils of widely varying degrees of ability the teacher simply does not have enough time. All too often it is merely a matter of trumping up extra tasks to keep the bright child busy while the harassed teacher struggles with a group of slow readers. As a result, gifted children tend to develop poor study habits and even to become behavior problems.

Acceleration. There remains the third method, acceleration. It was used somewhat indiscriminately in the 1920's, sometimes with unfortunate results, and in consequence has suffered from what Pressey terms "the maladjustment bugaboo" (*27*). But acceleration is administratively simple. It raises no problems of space or transportation. It puts no extra burden on the teacher. Careful studies of the results of acceleration have been made by Keys (*28*), by Terman (*8*, p. 264), and by Pressey (*27*). All reach the same conclusion: acceleration of one or two grades is highly desirable for the majority of gifted children *but must take place on an individual basis, after a careful study of the intellectual, social, and emotional factors.* These conclusions are supported by the studies of Witty and Bloom (*26*) and of Wilson (*29*). Keys considers that, when all other factors favor acceleration, physical development is not of paramount importance (*28*). Pressey, in particular, emphasizes the need for continued guidance of the gifted child and for early identification (*27*). He considers that entrance to school should be on the basis of total maturity rather than chronological age. The Educational Policies Commission of the National Education Association has accepted these conclusions so far as to recommend acceleration in suitable cases where homogeneous grouping is not possible (*16*).

Conclusions. The foregoing considerations point to the conclusions that (i) in many school systems the needs of gifted children are not being adequately met; (ii) provisions should be made for such children in the elementary schools; (iii) in the elementary schools, homogeneous grouping or acceleration is preferable to enrichment; and (iv) each gifted child should be studied individually before a decision is reached on the best method of providing for his or her needs.

Tentative Plan

A positive program that would embody these findings is needed. Such a program must be comprehensive; it must include all gifted children in the state. It must be flexible, so that it can be carried out in many different types of school. It must be simple and relatively inexpensive. It must not call for radical innovations in the existing administrative framework. And last, it must provide for close cooperation between psychologists and educators, to insure the most effective planning for each child.

The first step in such a program would be early identification. A group test of intelligence would be given to all children in the first grade and also to older children on their entry into the school system. All children exhibiting an IQ of 130 or higher

on this test would be given the Stanford-Binet Scale. To allow for chance variations on group tests, the Stanford-Binet would also be given to any child scoring below 130 on the group test whose teacher considered him or her to be of outstanding ability. The great advantages of identification in the first grade are (i) the earlier the guidance, the more effective, and (ii) the teacher's estimate is less likely to be affected by considerations of achievement. Every child with an IQ of 140 or higher on the Stanford-Binet would be classified as "gifted," or "talented," or "advanced," or whatever term might be preferred.

Each gifted child would then be given a comprehensive individual psychological examination, in addition to the Binet. At first, tests and procedures employed in the examination would be selected from those already standardized and in current use, but as soon as the plan was working smoothly situation tests of various kinds would be introduced. Some might be adapted from the situation tests developed by the British in World War II.

On completion of the examination, a discussion would be held with the parents and a decision reached regarding the child's placement. Unless there were strong contraindications, the child would be either placed in a special class or accelerated. A central registry would be kept at state headquarters of all gifted children, containing names, ages, school data, and psychological findings. The progress of each child would be periodically reviewed in conferences between parents and school officials.

A program of this nature has the advantage that it could be carried out within the framework of the existing school system. In certain instances, additional psychologists and administrative and clerical workers would have to be hired. The effort and expense involved would be small in comparison with the benefits that would ensue, both to the individual children and the state.

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The secret of education lies in respecting the pupil.—R. W. EMERSON.

New Ideas in Chemistry

JOHN LENNARD-JONES

After arrangements for the publication of this article were made, the regrettable news was received of the death of Sir John on 1 November 1954. Sir John, an eminent British physicist, was principal of the University College of North Staffordshire, England, at the time of his death. This article is based on an address that he gave before the Chemistry Section of the British Association for the Advancement of Science in Oxford, 2 September 1954. It is reprinted by permission from The Advancement of Science, *No. 42, September 1954.*

IT is the purpose of this article to describe some of the new ideas that have been introduced into the theory of molecular structure within recent years. These ideas have been drawn from physics, where they have been singularly successful in correlating the properties of atoms. The scope of the theory has been widened so that the physical and chemical properties of both atoms and molecules can be explained by the same unifying hypotheses. As the range of "explanation" of the facts of nature extends, the more mysterious become the properies to be ascribed to the ultimate particles of which matter is composed.

In the progress of science there are two processes at work. In the one there is the steady accumulation of factual knowledge, and in the other there is an attempt to interpret the facts in terms of unifying principles. It is the interplay of theory and experiment which leads to the most important advances, for often the interpretation of one sort of fact leads to the prediction of new results. The most impressive advances are produced when general hypotheses are put forward which bring different branches of science within the same comprehensive scheme.

There has been no more striking example of this than the recent unification of atomic physics and chemistry. When the British Association last met in Oxford in 1926, there seemed little relation between the subjects discussed in the section of physics and those discussed in the section of chemistry. The presidential address in the one dealt with the properties of atoms in absorbing and emitting light; that in the other, with the scope of organic chemistry. It did not seem possible at the time that both domains of knowledge might soon come within the same theoretical framework. Yet even during the Oxford meeting there were rumors of a new theory of the atom which was said to be brilliantly original. We did not know then that this new theory was to let in a flood of light on the behavior of molecules and illumine much that was obscure in the world of chemistry.

Orbit Theory of Atoms

In 1926 electrons in atoms were pictured as rotating in orbits around a central nucleus like a solar system. To fit the known properties of atoms it had been found necessary as well to assume that electrons had a magnetic moment. They were said to have *spin* and the axis of spin could set itself along or opposed to any applied field. The spin had thus two possibilities only.

On this basis Bohr had made an advance of great importance to the chemist by suggesting how the electrons in atoms should be assigned to the available orbits so that their chemical behavior might be explained. The striking achievement of this theory was to provide an understanding of the similarities in the behavior of atoms assigned by chemists to the same groups in the periodic table. The Bohr theory was greatly simplified by Pauli when in 1925 he proposed an important new property of electrons in matter. According to this, only two electrons in an atom can follow the same orbit, and of these one must have one kind of spin and the other the opposite spin. In other words, no two electrons in an atom can simultaneously have the same spin and occupy the same orbit.

The full significance of Pauli's hypothesis, now universally called the *exclusion principle*, was not fully realized when it was first announced. Its main purpose was to simplify the Bohr description of atoms or to suggest simple rules by which the same results could be achieved. But it has since become clear that it is of wide validity. It applies not only to atoms but also to the totality of electrons in a molecule, however many atoms that molecule may contain. Although first put forward as a guiding hypothesis in physics, it has become the cornerstone of chemistry. However complex a molecule may be, however large the number of electrons it

may contain, there seems to be an "awareness" by each electron of what all the others are doing and a strict adherence to the rule that no two must play the same role at the same time.

No theoretical proof has been given of this exclusion principle. It is injected into current theories as a working rule. It represents some deep property of electrons, and indeed of all matter, which is not understood. It implies an intimate interconnection between the constituent parts of molecules and between one molecule and another. The principle suggests that there is a connecting strand running through the material universe, so that strictly no part can be isolated—except in an idealized sense—from the rest. There are always influences at work conditioning the behavior and properties of the part and connecting it with its environment. These influences are more subtle than forces of repulsion or attraction such as exist between electric charges of like or unlike kind. These latter are of short range and cease to be effective beyond lengths of atomic dimensions, which are infinitesimally small. But the exclusion principle applies to all the electrons of a long threadlike molecule or a long strip of metal, so that each is influencing the behavior of the rest over distances that are millions of times greater than the sizes of the atoms which the metal contains. There seems here to be some aspect of nature of great philosophic content beyond the understanding of the physicist. He must be content to accept the implications without hoping to penetrate the mystery that is implied.

New Ideas

The Bohr atomic theory made clear the significance of complete shells of electrons in atoms and explained the tendency of some atoms to lose one or two of their outer electrons and of others as easily to assimilate them. Thus the type of bond known as the *electrovalent link* became understood in terms of the inner structure of atoms. Physics had successfully invaded the realm of chemistry. But the penetration was incomplete. The theory gave no indication why atoms of the same kind should combine, and so the whole field of organic chemistry was left untouched. It was the new theory of the atom that opened the way.

Stimulated by de Broglie's suggestion in 1924 that electrons, indeed that all matter, have wave properties, Schrödinger developed what has now become known as *wave mechanics*. This is to be regarded as a generalization of Newtonian mechanics, for it leads to Newton's laws when applied to heavy particles. There cannot be said to be a proof of the wave equation, for it was obtained by analogy, and its justification rests only on its success. The wave equation must be accepted as an article of belief or a fundamental postulate. It involves some strange ideas and some processes of calculation which cannot in detail be understood. Thus the solution of the wave equation for any problem is expressed as a function of the coordinates of the system and of the time, usually denoted by ψ, but this function is merely a means to an end. It is not possible to give to it any simple interpretation in terms of observables. It is to be looked upon rather like a ghost that produces material manifestations. One simple connecting link between ψ and observation is that for a given system the square of the amplitude of ψ, for a particular set of coordinates, gives the probability that a system has a particular configuration, specified by its coordinates. Rules have been devised whereby the probable values of dynamical quantities can be derived from ψ, and so the theory can be tested by comparison with experiment. In particular, the theory leads to a set of stationary states with definite energy values for atomic and molecular systems.

The wave theory of atoms was the first new important idea since the Bohr theory to prove of importance to chemistry. It was found possible to use with it the idea of electron spin and the exclusion principle as well. But the new theory brought other ideas in its train.

So far as we know, electrons are indistinguishable from one another. An interchange of any two in a molecule does not produce an observable change. This means that the probability distribution function of ψ^2 must remain unchanged when the coordinates of any two electrons (with regard to both space and spin) are interchanged. This places a restriction of vital importance on the function ψ. Either it must itself remain unchanged or it must change sign.

It is now necessary to distinguish between these two possibilities. The exclusion principle provides the answer. It has been found that if the exclusion principle is to apply to the system as well, the wave function must change sign when any two electrons are interchanged. One consequence of adopting this form of wave function is that the possibility of finding two electrons with the same spin at the same point of space is completely excluded. The importance of this to chemistry will appear presently.

It was not long before the new ideas were introduced into chemistry. In 1927 Heitler and London made the first application of these novel methods to the structure of a molecule and showed that the theory successfully accounted for the attraction of two hydrogen atoms for each other. It is true that the calculated value of the energy of formation of

the hydrogen molecule differed considerably from the observed value, but it was of the right order of magnitude, and it was evident that for the first time here was a promising start toward an understanding of a covalent bond. The method of calculation used by Heitler and London was similar to that already used by Heisenberg in dealing with the structure of another two-electron system, the helium atom. The secret of success lay in constructing a wave function that had the necessary properties to take into account the identity of electrons and the exclusion principle. The wave function in terms of space and spin coordinates was so designed that it changed sign when the two electrons were interchanged. We know now that other wave functions can be devised to have the required properties and that from them also calculations can be made of the energy of formation of the hydrogen molecule. The method used by Heitler and London was not the only one, or indeed the right one, to give a deep understanding of the forces at work in a molecule to produce stability.

One important new idea came out of the Heitler-London approach. Once their method of calculation was generalized by Pauling and others to more complex molecules, it emerged that there were different ways of obtaining a wave function with the correct symmetry properties. Further, it was found that if such wave functions were combined together as linear sums (the symmetry properties thus being preserved), the resulting expression gave a better representation of the system, for when used to calculate the energy it gave lower values and thus indicated greater stability. Now the component parts of the wave functions could each be described (roughly) as typifying a structural formula in the usual chemical sense. Thus emerged the idea that often a molecule cannot be described satisfactorily by any one classical structural formula. It must be regarded as a composite state, best described as a superposition of different structural formulas. This phenomenon was at first called "resonance," but the description is misleading, for it suggests actual oscillation between several alternative patterns, whereas the true state is intermediate between all of them and simultaneously involves in varying degrees the properties of each. The idea has proved of great value in chemistry in clearing up many phenomena that had previously remained obscure.

One weakness from a theoretical point of view is that the method tacitly assumes that a precise meaning can be attached to each structural formula, and that a single or double bond in different environments has the same significance. Actually it is not possible to describe even the simplest "single" bond, namely, the bond in the hydrogen molecule, accurately without having recourse to the idea of superposition of different conceivable configurations, ionic as well as covalent. Nor does the method make clear why it should be necessary to superimpose different electronic structures. It would be helpful if some understanding could be gained of the influences at work in molecules to produce properties which seem sometimes to correspond to one kind of structure, sometimes to another, and in some cases to be intermediate between those expected of two or more structures.

Molecular Orbitals

It is for this reason that another form of description of molecules has been found to be useful. When several electrons move in the *same* field of force, the wave function of the system can be expressed *accurately* as a product of functions of the coordinates of the individual electrons or as a sum of such products. Each such function corresponds to a distribution throughout the field of force and is called an *orbital,* because this term implies a generalization of the notion of an orbit.

It seemed natural, therefore, to begin with molecules on the assumption that the fields of the electrons on each other could be so smoothed out that all electrons moved in the same field. The problem was then soluble in principle, and although it was necessary in practice to make approximations concerning the form of solutions, this did not detract from the essential soundness of the approach. According to this method, the electrons could then be assigned in pairs to a set of functions, each of which represented a definite energy. Each such function depended on the space coordinates of an electron only, and for convenience it was called a *molecular orbital.* The energy of the system on this simple view was the sum of the energies of the individual electrons.

It should thus become clear that the term *molecular orbital,* as first introduced, represented any one of the set of solutions of a wave equation of a single electron in a certain averaged field. The term was thus synonymous with a wave function. The orbitals determined in this way had an important property. If two different ones were multiplied together and the product integrated over the whole of space, the result was equal to zero; they were said to be *orthogonal.* This feature has proved to be a necessary one for the application of the exclusion principle. It is as though in molecules, as in music, nature prefers a form of harmony in which the "notes" are compatible and the chords are pure.

The method proved to be powerful because the orbitals were found to have characteristic symmetry properties and could be divided into classes. Thus

it appeared that if a molecule possessed a plane of symmetry, then some orbitals were symmetric about this plane, while others were antisymmetric; that is, at corresponding points on each side of the plane the orbital had values equal in magnitude and opposite in sign. One important application of this has been made to conjugated molecules, for they usually have a plane of symmetry through the nuclei.

It should, however, be noted in passing that, while the orbitals have these different symmetry properties, the charge distributions appropriate to all the orbitals are symmetric about the plane of symmetry, for they are obtained from the square of the orbital values.

This simple form of molecular orbital theory has been used extensively and has helped to clarify much that was obscure in chemistry. It was used by the writer for diatomic molecules and applied with great success by Hückel to conjugated molecules. It has been developed and applied to various problems of structure by Mulliken, Coulson, Longuet-Higgins, the writer and their coworkers, and by many others. Recently Dewar has made numerous applications of the method and, in particular, has given a simple treatment of the color of conjugated molecules in terms of their structure.

The idea that an electron may be distributed throughout the whole of a molecule is at first strange to the chemist, who has been brought up to think in terms of localized bonds. But the principles of the wave theory force us to accept this view. When an electron is removed from a molecule by the absorption of light or by other means, it must be regarded as taken from the whole molecule and not from a localized part. The "hole" that is left is closely related to the pattern of distribution represented by a molecular orbital.

There has been a further development of the molecular orbital theory. In a real molecule the fields of force acting on different electrons are not exactly the same, because the exclusion principle forces the electrons into different orbitals, and the effect on one electron of the repulsive field of all the others varies according to the orbital it occupies. For this reason the wave function cannot be expressed accurately as a product, or a sum of products, of functions of the coordinates of each electron. Progress can be made if, in spite of this, the same form of wave function is adopted and individual orbitals determined in such a way as to give the "best" solution. By this is meant that solution which renders the calculated value of the energy a minimum. This method of calculation automatically introduces a form of averaged field on each electron, but it is different for each. This field is the one that would arise if *all the other electrons* were smeared out in space according to their orbital distribution. Progress in this stage of the theory has been made mainly since 1949 and it has been found that a new range of problems has come within its scope. Applications of the method made at Cambridge and Chicago hold promise that it will be able to deal not only with structure but also with ionization potentials of molecules and the interrelations of the one with the other.

There is further room for improvement. It will be necessary finally to take into account the difference between the averaged field, even though this is different for each orbital, and the actual field. It is this difference alone which prevents the theory from being accurate in all respects. Thus this method of calculation makes clear that the complexities of the theory arise entirely from the fields of repulsion between electrons, for these depend on the simultaneous position of two electrons and thus involve six coordinates. Successive refinements of the theory represent stages in the process of calculating the effect of one electron on the distribution in space of another. The problem is thus one of finding the effect of electrostatic forces on the spatial correlation of electrons relative to each other.

These complexities can be avoided and the physical significance of the principles determining molecular structure made clear if attention is first directed to some simple systems in which the particles move in the same field. The process of calculation follows closely that used for molecules by the orbital method.

Model 1. Because it can be used to illustrate some of the properties of butadiene, the first example to be considered is the problem of two particles of the same spin moving in a region of finite length a within which there is no force field. It is supposed that the particles are trapped within this region and that the boundary walls are such that there is no penetration. It is sufficient for a discussion of a problem such as this to concentrate on the behavior of the particles in the one dimension across the region. To simplify the discussion it will be assumed that the particles do not interact with one another by any direct force field. In this, as in all atomic and molecular problems, it is necessary first to find the appropriate wave representation of the particles. For a single particle moving in such a force-free field, the wave function can be any one of a set of sine functions, each fitting into the region in such a way that it has zero value at the two extremities. This implies that the half-wavelengths of the functions fit into the width of the region an integral number of times. To each function corre-

sponds to definite energy and the set can be arranged in an increasing sequence, E_1, E_2, E_3. . . . Since the particles have the same spin, the lowest energy of the two is $E_1 + E_2$, because they must occupy *different* orbitals. The wave function of lowest energy is sin $(\pi x/a)$ and the next lowest is sin $(2\pi x/a)$. These are the two occupied orbitals.

It is necessary now to take into account (i) the identity of the particles, so that each may occupy either orbital with equal probability, and (ii) the exclusion principle. Since the particles do not interact with each other directly, the correct wave function is proportional to the difference of sin $(\pi x_1/a)$ sin $(2\pi x_2/a)$ and sin $(2\pi x_1/a)$ sin $(\pi x_2/a)$, and this is an accurate solution satisfying the necessary conditions.

Once the wave function $\psi(x_1, x_2)$ has been obtained, it is possible to deduce the properties of the system. Thus the square of ψ, multiplied by $dx_1 dx_2$, is interpreted as the probability that one particle is between x_1 and $x_1 + dx_1$, with another at the same time between x_2 and $x_2 + dx_2$. From this we can derive the probable distribution of one particle relative to the other. Figure 1 shows the form of the wave function for a given position of one particle and the probability distribution of the second particle relative to the first.

It is evident that the second particle avoids the first and that its most probable position is between the first and the further boundary. It is to be noted that this does not arise from any mutual repulsion of the particles because it has been assumed that none exists. It arises only from the exclusion principle.

As a corollary to this example, it is possible to consider the case of four particles moving in the same one-dimensional field, there being two of one kind of spin and two of another. The second pair of particles can also be assigned to the two lowest energy states without conflicting with the exclusion principle. The wave function now consists of products of four sine functions, one for each orbital to which the particles are assigned. In addition it is necessary to introduce factors to indicate the spin to be associated with the particles in each arrangement. The complete wave function is so arranged that it changes sign for any interchange of electrons; it takes the form of a determinant.

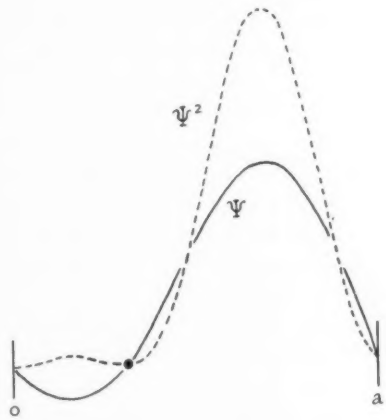

Fig. 1. Distribution of one particle relative to another owing to the exclusion principle.

For this system it is found that the distribution of one particle relative to another of the same spin is the same as before. One avoids the other. On the other hand, particles of opposite spin can move independently of each other. There is no correlation between them. The probability distribution of one relative to the other has a constant value across the region. This represents a feature which appears in all molecular systems. The exclusion principle does not itself require any correlation between electrons of opposite spin. Whatever correlation exists arises from other causes.

Model 2. A model that can be used to illustrate some of the properties of the benzene molecule is that of six particles moving in a ring of constant potential, three particles each of opposite spins. Again it is supposed that the particles do not interact with one another by force fields.

As in the first example, it is instructive to consider first the case of particles of the same spin. The wave function consists of three factors, each of which is a solution of the wave equation for a single particle moving in the ring. The solution of lowest energy has a constant value around the ring, say C, and the energy is zero. There are two solutions of next highest energy and they are sin φ and cos φ, where φ is an angular coordinate determining position. The wave function of the three-particle system is made up of a linear sum of terms such as

$$C(1) \sin \varphi_2 \cos \varphi_3,$$

indicating that one electron is assigned to each orbital. When this linear sum is arranged to change sign for any change of electrons, it proves to be proportional to

$$\sin (\varphi_1 - \varphi_2) + \sin (\varphi_2 - \varphi_3) + \sin (\varphi_3 - \varphi_1)$$

and this vanishes whenever two of the coordinates are the same. No two particles ever occupy the same place in the ring simultaneously.

The maximum value of the wave function occurs when the three particles are disposed at equidistant intervals around the ring. Thus the system tends to adopt trigonal symmetry, and this property arises solely from the wave representation of the particles

and the exclusion principle. The probability distributions of one particle for given positions of the others are shown in Fig. 2. Again we see the tendency of the particle to take up a position midway between the other two.

When there are three pairs of particles of opposite spin, each set of three of the same spin has the properties just described. In particular each system tends to arrange itself so that the three particles are equidistant around the ring. But there is no correlation between the two systems. They move around the ring independently of one another.

Model 3. Because of its interest in illustrating some of the properties of carbon and complete octets of electrons, it is profitable to examine in a similar way first a system of four particles, each of the same spin, confined to move on a spherical surface, and then a system of eight particles.

Treatment on similar lines to that just given shows that the four particles of a given kind of spin tend to arrange themselves symmetrically on the sphere so that the lines joining them form a regular tetrahedron. This represents the most probable distribution. Other arrangements can occur, although with less probability.

Thus we see in this example the emergence of some of the tetrahedral properties of carbon, for in its tetravalent state it can be regarded as having four electrons somewhat in the way described, although the problem is more complicated because of electrostatic forces.

As in the other example, the two sets of four particles each of the same spin tend to adopt tetrahedral configurations among themselves, but the exclusion principle does not cause any correlation between the two sets.

Some correlation is produced by electrostatic forces but it is weak compared with that just described. Calculations show that these forces increase the probability that four electrons of the same spin will take up a tetrahedral arrangement. Correlation between electrons of opposite spin is so slight that a complete octet of electrons may be described as consisting of two loosely coupled tetrahedrons. Of course it must be emphasized that while a tetrahedron represents the most probable configuration of each set of four, there is considerable flexibility about that arrangement.

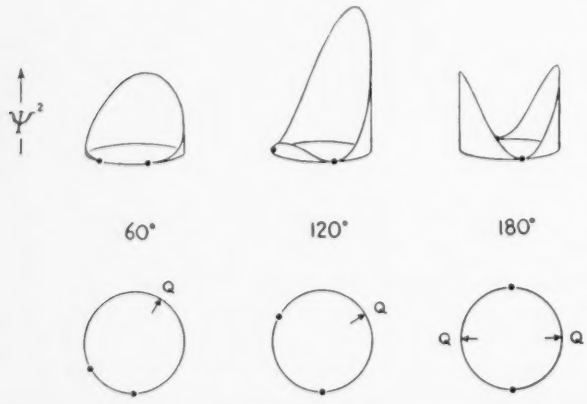

Fig. 2. Distribution of a particle relative to two others.

These models bring out a property that holds for all electronic systems, whether they are atoms, molecules, or solids: *Electrons of like spin tend to avoid each other.* This effect is most powerful, much more powerful than that of electrostatic forces. It does more to determine the shapes and properties of molecules than any other single factor. It is the exclusion principle that plays the dominant role in chemistry. Its all-pervading influence does not seem hitherto to have been fully realized by chemists, but it is safe to say that ultimately it will be regarded as the most important property to be learned by those concerned with molecular structure.

Examples of Molecular Structure

In actual molecules there are three main factors determining structure: (i) the exclusion principle, (ii) the field of the atomic nuclei on each other and on the electrons, and (iii) the forces of the electrons on each other. In methane, for example, the electrons are attracted to the central carbon nucleus and to the hydrogen nuclei; also, the hydrogen nuclei repel one another. Under their own forces alone the hydrogen nuclei would tend to take up a tetrahedral arrangement. The electrons may be regarded as already suitably disposed to receive them and to provide the maximum attraction when the nuclei are most deeply imbedded in the electron cloud. In such a system, the exclusion principle, the repulsion between the electrons, and the repulsion between the nuclei all act in unison to produce the same effect. Whereas in the neonlike structure there is little correlation between the sets of electrons of different spins, in methane both sets are attracted by the hydrogen nuclei, and the two tetrahedrons tend to be drawn into coincidence. Thus the eight electrons may be regarded as arranged in pairs in a tetrahedral manner *relative to each other* but constantly interchanging their positions. This is the interpretation to be given to localized bonds.

The structures of molecules such as ammonia, water, and hydrogen fluoride are closely related to that of methane. In each there is an octet of electrons. In ammonia the two tetrahedral distributions of electrons are held by the three hydrogen atoms. Relative to the three NH "bonds," there is a "lone

pair" projecting out in a direction approximately at a tetrahedral angle to them. Figure 3 gives some indication of the reason for this. The angles between the bonds and the lone pair differ from tetrahedral ones only because of the forces between the nuclei and the electrons near them.

Similarly the water molecule may be pictured as having two pairs of electrons projecting out in directions nearly at tetrahedral angles to the two OH bonds, and the HF molecule as having three lone pairs of electrons projecting out behind the HF bond. Figure 3 again may be used to illustrate this, the diagram showing equally well the most probable distribution of three electrons of the same spin when the position of the fourth is given.

The great significance to chemistry of these distributions of lone pairs is that they provide electron-rich regions and so are particularly reactive to electrophilic agents. The type of attraction between molecules or groups of molecules known as the *hydrogen bridge* may be attributed to the interaction of a positive center and an electron-rich distribution. These regions also contribute substantially to the electric moments of the molecules and thus have a major effect on their physical properties. Thus the characteristic behavior of these and similar molecules may, to a major extent, be traced back ultimately to the operation of the exclusion principle.

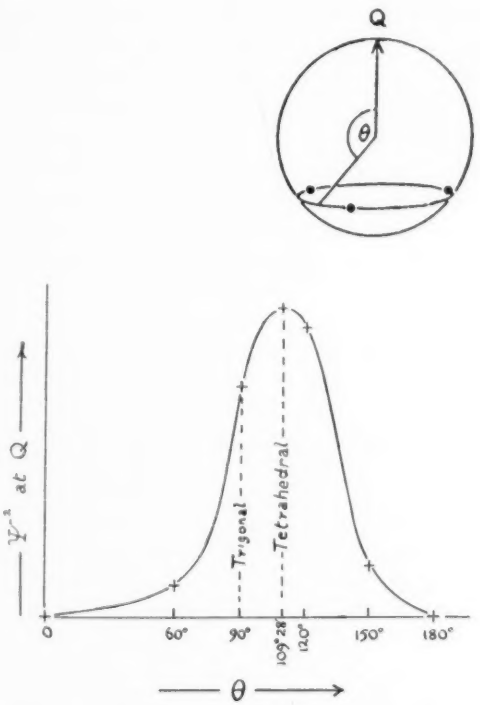

Fig. 3. Distribution of a particle relative to three others on a spherical surface.

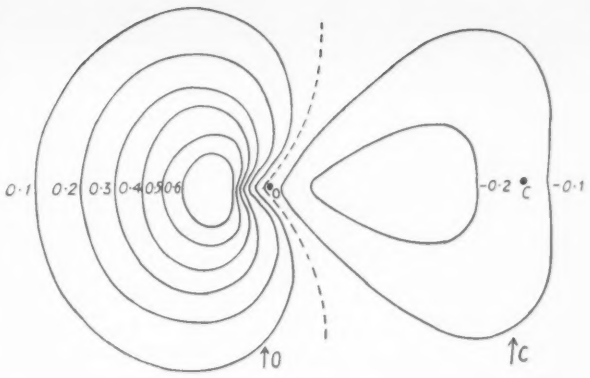

Fig. 4. An electron distribution in carbon monoxide, showing a concentration of charge outside the nuclei.

Were it not for the exclusion principle, all the electrons would crowd into the orbital of lowest energy, and this would mean that they would tend to be near the nuclei or in those regions between them where the field is strongest. But the exclusion principle forces electrons into parts of space where they are remote from some of the nuclei. Thus, in the molecule of carbon monoxide it is found that the ten outer electrons are to be assigned to five orbitals, of which only one gives a concentration of electron density between the nuclei. In one of the others the region of greatest concentration is near the oxygen nuclei on the side remote from the carbon atom. This is shown in Fig. 4. The contours show where the orbital has the same value.

In another orbital there is a similar distribution near the carbon nucleus on the side remote from the oxygen atom. These latter distributions each make large contributions to the electric moment of the molecule but they are in opposite directions and nearly cancel out. Although carbon monoxide has a small over-all electric moment, there are asymmetries of charge distribution within the molecule which have chemical significance.

Conjugated Molecules

One of the most interesting and profitable applications of the new ideas has been to the structure of conjugated molecules. The nuclei of a conjugated system as a rule lie in a plane. The orbitals occupied by electrons in such a molecule can be divided into two distinct types. In the one the orbital distribution is symmetric about the nuclear plane; in the other it is antisymmetric—that is, the value to be associated with the orbital at corresponding points above and below the plane is the same in magnitude but opposite in sign. It follows that in the latter case the orbital has a zero value at all points in the nuclear plane. It is said to have a nodal plane.

In the language of chemistry, the electrons in the single bonds of a conjugated system belong to the symmetric type, the electrons taking part in conjugation to the other. The former are called σ-electrons, the latter π-electrons. Carbon atoms each provide one electron of the π-type, nitrogen and oxygen atoms sometimes one, sometimes two, according to the other atoms combined with them.

It is a consequence of these different symmetry properties that the π-electrons can for many purposes be dealt with separately. The four π-electrons of butadiene can be dealt with theoretically very much as are the four particles in a one-dimensional box, described in a foregoing section. In the normal state of the molecule the electrons consist of two pairs of opposite spin, these being assigned to the two lowest orbitals of π-symmetry. These orbitals have properties along the chain somewhat similar to the sine curves of the model; in particular, electrons of the same spin tend to avoid each other.

The difference between the model and the molecule lies in the kind of field in which the electrons move. In the molecule the carbon nuclei attract the π-electrons, and the saturated electrons repel them. The net effect is that the π-electrons move in a field that changes periodically along the molecule, but they are in fact trapped in a boxlike domain. The fluctuations in the field along the box do not alter fundamentally the wave representations of the electrons. There are thus three main factors at work in determining the most probable configuration of the four π-electrons: (i) the nuclei, which attract the electrons, (ii) the exclusion principle, which tends to keep electrons of the same spin apart, and (iii) the mutual repulsion between electrons whatever their spin.

The attractive field of the carbon atoms will tend to keep one electron near each of them on the average, but the exclusion principle influences the arrangement of spins. If, as has now become usual, spins of one kind be denoted by the symbol α and those of the opposite kind by β, the combined effect will be to render the arrangements α, β, α, β and β, α, β, α more probable than those of the type α, β, β, α with two neighboring spins. It may happen that two electrons are simultaneously near one carbon atom, provided that the spins are opposite. Such an arrangement might be written $\alpha\beta$, —, α, β with others of a similar kind. This would correspond to what is usually called "an ionic structure." The mutual repulsion of the electrons will, however, reduce the probability of these configurations.

These general features have been confirmed by detailed calculations of the electronic structure of butadiene. Thus, if an electron of given spin is assumed to be at a fixed position near one of the terminal carbon atoms, say C_1, then the probability of finding another of the same spin near the same atom is very small, the probability rises to a maximum on carbon atom C_3 and has a smaller probability of being at the other two. On the other hand, the probability of finding an electron of opposite spin has maxima at the second and fourth carbon atoms, C_2 and C_4.

This view of the inner structure of the molecule permits some insight into the processes at work in producing the property of alternating polarity. Suppose that under the influence of a reacting agent of an electrophilic kind the attractive field at a terminal atom is increased. There will then be a tendency of other π-electrons to move toward it; and the α- and β-electron systems may behave differently. Thus, if the sequence of spins is α, β, α, β, then the β-electron from the second carbon atom can move toward the first under the influence of the field without conflicting with the exclusion principle. The β-electron on the fourth atom can do likewise, but the α-electron on the third carbon is held off. Thus we can regard the α-system as coupled together and the β-system as coupled together, but with only loose coupling between the two sets owing to electrostatic forces.

The process of shift of π-electrons in electric fields is illustrated by Fig. 5, which shows the distribution of π-electrons in carbon monoxide. The contours show equal values of the orbital, rising to a maximum value above and below the oxygen nucleus. It is probable that there is a similar shift in conjugated systems wherever the —C═O group occurs. This permits a corresponding shift of all electrons, of the same spin on alternate atoms.

A similar picture of the unsaturated electrons in

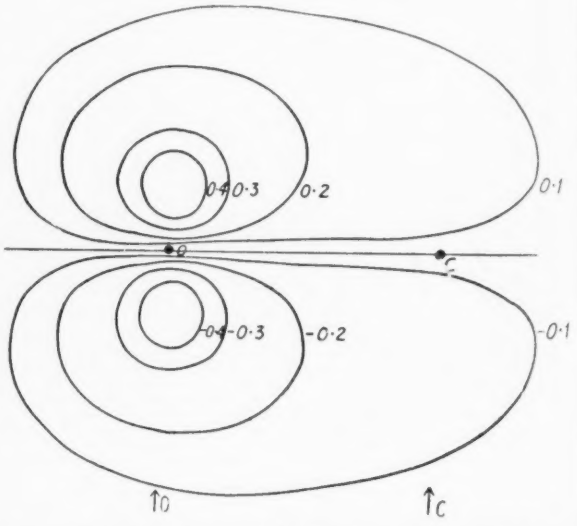

Fig. 5. Distribution in a π-orbital in carbon monoxide.

benzene can be drawn on the basis of the model of six electrons moving in a ring considered earlier. The property of two sets of three electrons of the same spin, each coupled in trigonal symmetry within itself but moving independently of each other, is modified by the electrostatic fields of nuclei and electrons. These fields will tend to produce an arrangement of alternating spins around the ring, and there are two ways in which this can happen. As in the case of butadiene, an increase of attractive field at any one atom, whether the electron spin be α or β, will tend to cause a shift toward it of the set of electrons of the other kind of spin, that is, of the electrons in the ortho and para positions. These positions will thus tend to become deactivated. If, on the other hand, the electron at one carbon is repelled by the field of another group, then the set of which it is a member will move away and cause enrichment at ortho and para positions relative to the meta positions.

The Elusive Electron—Two Points of View

It is not to be assumed that such a picture of benzene, or of butadiene, is sufficient to understand fully how the molecule will behave in all conditions. The properties of electrons in molecules are too elusive for that. We cannot say with certainty that an electron is here or is there. The electron density in a particular part of a molecule may on the average add up to one unit, indicating a charge equal to that of one electron, but the charge density in that region is made of many parts, as though the electron were playing many roles at the same time.

Two analogies may illustrate the dual behavior of electrons. In some forms of dance, such as Scotch reels and square dances, there are four partners and for most of the time the dancers are to be seen in pairs at corners of the square performing some kind of figure. But there is constant interchange of partners, and the identity of the pairs at any particular position does not remain the same. If any one dancer were to fall out, the gap created in the movement would appear first at one corner of a square, then at each of the others in turn. The "hole" thus left might be regarded as equally divided between the four corners of the square and thus as affecting the whole equally. The removal of an electron from a molecule may similarly be looked upon as leaving a "hole" in the whole system.

We might also compare a molecule with a layer cake. There are two ways of dividing up the cake. It can be taken apart layer by layer, each with its own characteristic flavor. Or it can be cut into segment portions in the normal way. Either method could at each stage give the same fraction of the whole. Electrons removed from a molecule by the absorption of light leave according to the layer method. Those removed by the chemist in a reaction are usually drawn as from a segment portion. But there is one important respect in which the analogy fails. When a portion of a cake is removed, the rest remains as it was. In a molecule this is not so. When an electron is removed by taking away an atom or part of the framework, the rest of the molecule adjusts itself to a new set of layer patterns. These patterns are the molecular orbitals appropriate to the new framework.

These properties of electrons may be illustrated by one or two examples. We have already discussed the structures of butadiene and benzene from one point of view. It is instructive to consider them from another. If one of the π-electrons in butadiene could be trapped by some means, say the one on carbon C_1, the other three would distribute themselves among carbons C_2, C_3, and C_4 as though they formed an allyl radical. They would occupy the two orbitals lowest in the energy scale, with two in the lower one and one in the upper. The upper one, like the upper one in the box model considered earlier, has a node at the center; that is, the electron in it avoids the center atom (C_3). Let us next suppose that the trapped electron of butadiene is released and is then strongly repelled along the chain by some nucleophilic agent. In the limiting case when it is forced on to the allyl-like residue, the exclusion principle obliges it to occupy the upper orbital, that is, to divide itself equally between carbon atoms 2 and 4. These would then become reactive to electrophilic agents.

In a similar way an electron on a carbon atom in benzene, say on C_1, if forced by some external agency on to the rest of the molecule, would be obliged to take up an orbital belonging to the pentadienyl residue. A simple, but approximate, calculation of the orbitals of this residue leads to the result that the one which is available, and lowest in the energy scale, has zero values on atoms C_3 and C_5, the meta positions. An electron in this orbital divides itself equally between atoms C_2, C_4, and C_6, the ortho and para positions. Although this state of affairs represents an extreme case, it illustrates the tendency of the benzene nucleus to become activated preferentially at the ortho and para positions when an electron is repelled from one carbon atom into the rest of the ring.

The effect of taking electrons out of the ring can be illustrated by considering what happens when the CH_2^+ group is attached to form the benzyl ion. A simple orbital treatment shows that the six electrons redistribute themselves among the seven centers in such a way that the ortho and para positions

are deactivated, the meta positions to a first approximation being unchanged.

Present Position and the Future

It has not been the purpose of this article to detail the methods of calculation that have been developed to deal with molecular structure but rather to discuss their physical content. An understanding of the physical ideas involved may help the experimenter to appreciate the processes at work in determining the way chemical reactions take place and the theorist to see the significance of the difficulties that arise in his calculations. There has been remarkable progress in the application of the wave theory to the structure of molecules, and the advances made have inspired general confidence that the principles governing the behavior of molecules are now known and that what is required is a rigorous method of calculation in accordance with these principles. It appears from what has been said here that the main difficulty in the theory is in the calculation of the effect of the repulsive fields of the electrons on each other. Other aspects of the theory do not present serious obstacles to calculation. It is because of this outstanding mathematical difficulty that the successes of the theory up to date have been mainly of a qualitative kind. Calculated values of observable quantities are as a rule approximately the same as those determined by experiment, but the stage has not been reached when computation can take the place of practical determination.

Nonetheless, the ramifications of the theory have been far-reaching. The covalent bond, which for so long was a mystery, is now understood. The gradations between covalency and electrovalency have become clear. The factors determining the shapes of molecules have become more precise. The properties of conjugated molecules have been found explicable in terms of the same principles that determine the structures of other systems. A further success of the theory has been in bringing into close relation to each other the chemical and spectroscopic properties of molecules. It is now possible to understand the conditions under which molecules may show color, and promising progress has been made in advancing a theory of dyes. Much remains to be done, but the prospect of further advance is bright.

In justification of the theme of this article (*1*) some words written by Faraday to Clerk Maxwell may in conclusion be quoted.

> When a mathematician engaged in investigating physical actions and results has arrived at his own conclusions, may they not be expressed in common language as fully, clearly and definitely as in mathematical formulae? If so, would it not be a great boon to express them so—translating them out of their hieroglyphics that we might work upon them by experiment?

It may not be possible for the theorist of today to attain an ideal so clearly set forth, but the task remains clear and insistent, now as then, that the attempt be made to express as simply and clearly as possible the physical meaning of the mathematical theories that have swept through the whole of chemistry.

Note

1. I am indebted to S. C. Nyburg for working out and drawing Figs. 1, 2, and 3, to R. C. Sahni and the Faraday Society for permission to use Figs. 4 and 5, and to P. P. Manning for allowing me to refer to his unpublished calculations on butadiene. I am also grateful to H. D. Springall, I. N. Sneddon, and S. C. Nyburg for reading this article in proof and for their valuable suggestions.

There seems to be a natural law that regulates the advance of Science. Where only observation can be made, the growth of knowledge creeps; but where laboratory experiments can be carried on, knowledge leaps forward.—FARADAY.

The Language of Science

CHARLES E. WHITMORE

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A GOOD many people, I dare say, have a rather indefinite notion that scientists have a language of their own, a language that is largely the result of careful choice, perhaps regulated by some general body, but certainly more precise and more scrupulous than the language of every day. What we actually find on closer inspection is that every scientist uses certain terms appropriate to his special activity, many of them not at all familiar to workers in other fields, although some terms may be widely understood. All the terms, however, are imbedded in a wider language, which is in the first instance whatever the native tongue of the scientist happens to be. Instead of a single unified scientific language, what we have is an assortment of technical vocabularies of the most various composition, growing and changing, just as any elements of language do. Whether or not this state of affairs should or may be modified, it is advisable, with the help of a few case histories, to see how it has come about and what forces have been concerned in producing it.

The idea that the problem of scientific expression could be circumvented by using a pure notation of concepts, almost or wholly divorced from words, has often been put forward but never realized. The business of ordinary life, and even much practical scientific activity, simply could not be carried on by any such means. Perhaps an approach to such a condition might be found in the group of mathematicians who gathered around Banach in the years before World War II, of whom it is recorded (*1*) that they spent long hours every day in two coffee houses, discussing problems over coffee or beer; but it is not likely that they ordered their drinks in technical or symbolic terms. After all, symbols cannot be used until their meaning has been ascertained and expressed in ordinary language. It is significant that number has always had both words and symbols: we write in numerals, but we count in words, of which every language has its own set. We have to explain the difference between signs for quantities and signs for operations, as well as the different meanings assigned to a symbol in different contexts, as a part of the general educational process, which is usually carried on in the native tongue of those concerned.

Midway between the purely mathematical treatment of certain portions of physics and the descriptive treatment appropriate to many portions of geology and biology is the system of symbols that Berzelius introduced in 1819 as a means of bringing order into the growing complexity of chemistry. His use of index numerals to qualify the letters that denoted the elements was objected to as differing from the use of exponents in algebra, and the juxtaposition of letters to indicate compounds was declared to be offensive to mathematicians because it confused addition with multiplication; but Berzelius declined to be impressed by these arguments. As late as 1837 Dalton declared that the new symbols were horrifying, and that a young student might as well learn Hebrew as make himself acquainted with them; but they were needed, and with a few modifications they won general acceptance (*2*). In actual use, however, they remain symbols within a context; a writer may use Pt or platinum according to his convenience, but he does not, in connected discourse, use symbols exclusively, nor does anyone mistake the periodic table for a piece of connected prose. Berzelius, it may be noted, wrote his explanatory text in French, the language best calculated to win the attention of chemists at that time.

A curious sequel to this controversy occurred in 1864, when Benjamin Brodie delivered before the Chemical Society a lecture on "The calculus of chemical operations," which he reissued in 1880 as a slim volume entitled *Ideal Chemistry*. He declared that the atomic theory

> . . . has proved itself unable to deal with the complicated system of chemical facts which has been brought to light by the efforts of modern chemists, and has not succeeded in constructing an adequate, a worthy, or even a thoroughly useful representation of those facts,

and that

> . . . the views of chemists as to the use of the apposition of letters as the symbols of combination are of a very vague character.

The absence of any specific symbols for combination was indeed his major objection; but the "operations" by which he proposed to reduce each element to the condition of a perfect gas and the Greek letters by which he sought to symbolize his "ideal" elements are anything but readily intelligible, and it does not appear that anyone was persuaded to adopt them.

Leaving the question of pure notation, we must turn to a consideration of the words that make up the English scientific vocabulary—to what classes they belong and where they have come from. In an old science like astronomy we have words such as *planet, comet,* and *eclipse* from Greek, *zenith* from (misunderstood) Arabic, *orbit* and *satellite* from Latin, all of which have long passed into common usage; and some of the terms for ordinary chemical substances undoubtedly go back to remote antiquity. We may note in passing that *satellite* was at first the singular *satelles,* with the plural *satéllites,* introduced by Kepler in 1611 and used in English by Pope more than a century later:

> Or ask of yonder argent fields above
> Why Jove's satellites are less than Jove?

Soon, however, the influence of the familiar ending *-ite* led to the acceptance of *satellite* as a singular, which it has remained to this day. The names of many common substances may well go back to the dawn of civilization in Mesopotamia and have passed through many stages of linguistic change and popular etymology. To trace such changes is an interesting occupation, even when much of it must be conjectural; but our concern here must rather be with the introduction of fairly recent terms under known conditions.

It is very difficult to invent an acceptable new word out of whole cloth; Van Helmont's oftencited *gas* is one of the very few authentic cases. Recourse is usually had to a word borrowed from some existing language, either directly or with such modification as analogy or euphony may suggest. Greek and Latin have naturally been the great sources, with some Arabic infusion in astronomy and chemistry; geology has drawn on Celtic and Slavic tongues; and there is no reason why a word should not be taken from any known language if it happens to hit the borrower's fancy, provided that it does not look too startling. When suitable single words are not available, recourse may be had to the so-called "acrophonic" forms, familiar in such trade names as SOCONY and NABISCO, and, in another field, UNESCO. Organic chemistry, when it was multiplying discoveries in the early 19th century, yielded many such condensations: for example, *paraffin* from *par*um *affin*is, or *aldehyde* from *al*cohol *dehyd*rogenated; or, odder yet, *mercaptan* from *mer*curium *captan*s for that unpleasant class of compounds of which the most familiar is the active principle of a skunk's odor—a name that is pretty close to a conundrum. Today we have such words as *radar* and *loran,* along with less acceptable coinages, some of which seem consciously freakish and unlikely to attain permanent acceptance.

In general, new names are devised to designate new substances, new instruments, and, less frequently, to name new principles or concepts. Since one discovery often leads to another in the same field, groups of words are built up on the same general model for groups of related objects. It is unnecessary to do more than mention the numerous terms that are made from *-scope* and *-meter*; they are formed as needed, and their use gives rise to no problems. A less simple case is the naming of new chemical elements in the early 19th century. It was agreed, in general, to use the ending *-on* for nonmetals, *-ine* for the halogens, and *-ium* for metals, appended to the name of the substance from which they were extracted (*sodium, potassium*). Davy wrote, during his researches of 1808:

> Had I procured the metallic substances I was in search of, I should have proposed for them the names of *silicium, alumium, zirconium,* and *glucinium.*

The first of these later became *silicon,* by analogy with *boron* and *carbon*; the second, altered by Davy to *aluminum,* became *aluminium* for no clear reason except that, as a writer in the *Quarterly Review* declared, it had a more classical sound. Since all the Latin names of metals ended in plain *-um* (*aurum, ferrum*), and four modern names (*lanthanum, molybdenum, platinum, tantalum*) do the same, the logic of this argument is hard to follow; but in Great Britain and Germany the longer form is still preferred.

All this, it is clear, did not succeed in producing a uniform terminology. There were still the common names of the oldest known metals; there were *oxygen, hydrogen,* and *nitrogen*; there were *arsenic* and *sulfur*; there were *zinc* and *bismuth,* of unknown origin. When, in the 1860's, new metals were discovered by spectrum analysis, it was natural to name them from the colors of their chief spectral lines, so that we have *cesium, indium, rubidium,* and *thallium.* When Lockyer discovered evidence of a new element in the solar spectrum, it was natural to call it *helium,* since there was no way of ascertaining its properties; and when it was later found on the earth, and shown to be a member of the family of inert gases, it was too late to be

troubled by the fact that those gases were being given names in *-on—argon, neon,* and the rest—and no attempt was made to rename it. An earlier anomaly, however, has had a somewhat different fate. *Tungsten,* named in Swedish "heavy stone" from the weight of its ore, continued peacefully under that name until 1949, when it was proposed to rename it *wolfram,* after one of its ores and the name it has always had in Germany. But *wolfram* itself is of uncertain etymology, and the change would only mean substituting an unfamiliar label for one that has long been in use; hence, it is not surprising that 3 years later *tungsten* was "reinstated as an acceptable synonym," and it will probably continue to hold its place in our technical vocabulary, even though the chemical symbol for it has been, and is, W.

A conspicuous case in which care in the selection of new words was exercised is that of Faraday when he wanted them to designate the new phenomena of electrolysis and turned for help to Whewell, already known as a coiner of words in the fields of mineralogy and geology. Faraday's first biographer wrote (*3*):

> No man ever felt this tyranny of symbols more deeply, and no man was ever more assiduous than he to liberate himself from them, and the terms which suggested them. Calling Dr. Whewell to his aid in 1833, he endeavored to replace by others all terms tainted by a foregone conclusion.

Whewell replied with *anode, cathode, ion, anion,* and *cation,* which Faraday accepted with enthusiasm and proposed to the Royal Society in January 1834. Some months later he wrote to Whewell (*4*):

> I have had some hot objections made to them here, and found myself very much in the condition of the man with his Son and Ass, who tried to please everybody; but when I held up the shield of your authority it was wonderful to observe how the tone of objection melted away. I am quite delighted with the facility of expression which the new terms give me, and shall ever be your debtor for the kind assistance you have given me.

Whewell himself was not one to be deterred by objections. He declared (*5*, vol. 2, pp. 182–83):

> Even violent philological anomalies are soon got over, if they are used to express important laws, as we see in the terms *endosmose* and *exosmose*; and therefore there is little reason for shrinking from objections founded in ignorance against words which are really agreeable to the best analogies.

Acceptance of the new terminology, however, was not at first widespread; Tyndall, writing in 1868 of Faraday as a discoverer, could say that *anode* and *cathode* enjoyed only a restricted currency, and that the other terms were even less frequently employed. This was in part because Faraday worked largely by himself; when more workers came into the field the terms were ready for them, and today they are in general and unquestioned use. It is not true, however, that they can be kept as free of interpretation as Faraday had hoped. This proved to be the case when Arrhenius developed his theory of solutions; but it is evident that an adequate term need not be perfectly neutral, provided that its object can be effectively identified. It still makes sense to talk about atoms, although they are no longer what they were thought to be a century ago; and various views about them have been entertained without making it necessary to give up the word. It is only when opinions get completely away from the original meaning, or when views become contradictory, that a term has to be abandoned; and even then it may be redefined and kept in use if it still appears serviceable.

Contrasted with Faraday's orderly procedure is the casual and often inconsistent way in which the words denoting the "new particles" of recent physics have originated. As far back as 1891 Johnstone Stoney declared that it would be convenient to use the term *electron* for the assumed basic negative charge on an atom; it won considerable favor and was firmly established by J. J. Thomson's experimental work in 1897, replacing the term *corpuscle* which Thomson had previously used. Its meaning had also slightly shifted; Stoney had thought of the charge as inseparable from the atom, whereas Thomson had shown that it could be detached. When the corresponding positively charged particle was accepted as a fact and found to be identical with an ionized hydrogen nucleus, it was appropriately named *proton,* as coming first in the atomic sequence. The subsequently discovered hydrogen isotope of double weight was named *deuteron* or "second," the shorter *deuton* not making its way into use. For some years, then, protons and electrons were the accepted ultimate particles, the *-on* in each case being the Greek neuter ending.

By 1932, however, new discoveries were making the situation more complicated. The uncharged particle, discovered in that year, was appropriately named the *neutron,* a Latin form with a Greek ending; the Greek would have been the longer *oudeteron,* but the shorter hybrid was acceptable enough and soon became standard. At about the same time evidence accumulated for the existence of a positive electron, for which Anderson, its discoverer, proposed the contracted form *positron,* with the added suggestion that the negative electron should "from symmetry considerations be

called a *negatron*." (*6*). The contraction is, of course, arbitrary; it is not clear, at first sight, whether the division should be *posi-tron or posit-ron*. If the latter (analogous to *posit-ive*), the *r* is an insertion, and the preferable form would be *positon*; but this, although I have occasionally seen it, seems never to have become current, and *negatron* seems to have won no acceptance at all, *electron* being too firmly established. At any rate, the adoption of the form *positron* led, as we shall see, to the possibility of a misunderstanding.

Meanwhile a new factor in the situation appeared in June 1931, when Lawrence announced a method for producing high-speed hydrogen ions (*7*)—the very modest entrance of an epoch-making innovation. Further statements appeared during the next 3 years, in all of which the new device was referred to simply as "the magnet" or "the apparatus." In September 1935, however, an account of improvements thus far made included this significant footnote (*8*):

> Since we shall have many occasions in the future to refer to this apparatus, we feel that it should have a name. The term "magnetic resonance accelerator" is suggested. In this, the last two words imply the essential principle of operation, while the first word is added to distinguish it from the apparatus of Sloan and Lawrence, which can be called a "linear resonance accelerator." The word *cyclotron,* of obvious derivation, has come to be used as a sort of laboratory slang for the magnetic device.

This statement seems clear enough; yet it is not so simple as it appears. The word *cyclotron* can hardly be called slang; it is a perfectly good Greek compound, although not everyone has found its derivation obvious. The Greek ending *-tron* indicates a means of doing something, as *arotron,* "a means of plowing, a plow"; *semantron,* "a means of signaling"; and the like. Since the new machine caused particles to revolve in circles, it was naturally and properly called a cyclotron. This ending has nothing to do with the *-tron* which appears in the names of particles; but various writers with more enthusiasm than knowledge have assumed that the two are identical. Thus I find in a recent book on terminology (*9*) this statement:

> From *electron* the *-tron* has been borrowed and used by analogy to form such words as *cyclotron, betatron*; it has no meaning other than being part of the word *electron.*

Actually there is no analogy here; *electron* is not made up of *elec-* and *-tron,* any more than *electric* is made up of *elec-* and *-tric*. The correct account is given, for instance, by J. M. Cork (*10*):

> The word *cyclotron,* although rhythmic and pleasing in sound, might be objected to by scholars of Greek since it comes from "cycle," meaning a circle, and "tron," a suffix meaning an agent or "that which does." It might be thought that the machine itself was describing circular paths.

But was this mistake ever really made?

At this stage we may say that two main lines of terminology were well established. *Cyclotron* was followed by a whole progeny of similar terms—*betatron, bevatron, calutron, isotron, synchrotron, cosmotron, colossotron,* the last rather suggestive of a circus poster, but all more or less acceptable. On the other hand, the discovery in 1936 of the so-called "X-particle" led to a flood of queer proposals —*dynatron, barytron, penetron, yukon*—before the more sensible term *mesotron,* which was soon shortened to *meson,* parallel with *proton* and *neutron*. The subsequent discovery of at least two varieties has led to the forms *mu meson* and *pi meson,* now, it appears, in process of being shortened to *muon* and *pion,* understandable as labels in this particular field of study (*11*). Finally, the hypothetical neutrino gets its name because its postulator, Fermi, happened to be an Italian and applied the native diminutive form to something much smaller than a neutron (*12*). The name seems appropriate to a somewhat different sort of particle from the rest and has persisted, even though it adds one more touch of diversity to an already diversified nomenclature.

From these assorted examples, we can draw at least two conclusions: (i) A word that applies to an object in general use will be adopted, whatever its origin. (ii) It makes little difference who introduces it, if it seems to meet no real need. In 1865 the chemist Hofmann proposed the name *crith* (from the Greek for a barley grain) to denote the weight of a liter of hydrogen under normal conditions, but it never became current. Perhaps the sound of the plural *criths* seemed unpleasant, although we have no trouble with *myths;* but it is more likely that there was no demand for that particular unit and, hence, no need of a name for it. Again, the objections of a particular user have little weight if a term seems desirable. Whewell disliked *tertiary* as a geologic term, but it prevailed nevertheless. Lord Kelvin's biographer records (*13*) that he expressly avoided certain terms in current use, and that he insisted that the common noun *ampere* should have no accent, although "he would never sanction the vulgarism of *ammeter* for *amperemeter*." Unfortunately, in actual use *ampere* comes into association with the monosyllables *volt, watt,* and *ohm,* and inevitably is clipped to *amp,* with *ampmeter* quite as naturally becoming

ammet by assimilation. Even the authority of Kelvin was powerless against such a natural working of linguistic habit.

Once a new word has been generally adopted, it seems to be a matter of chance whether or not the name of its inventor is remembered; even the dictionaries seem very casual about the matter. A later extract from Whewell gives us a suggestion of what may well happen. In a letter of 1862 he remarks (*5*, vol. 2, p. 426):

> I have been interested by the process of laying a telegraph wire almost from my door to Holland. The point where it leaves the shore here is only a quarter of a mile hence. I found the operator at the station employed in testing, as he told me, the conductivity of the wire. I was glad, for the sake of scientific propriety of language, to find that the word which I recommended years ago has made its way and established itself in the working vocabulary of galvanism as an art.

He was well aware that it is the general use of the thing signified that makes a word last. He had written in 1835 (*5*, vol. 1, p. 227):

> I begin to think it is both officious and unprofitable to draw up plans and to expect other people to work upon them, and I shall hesitate about proposing any more plans of notation and nomenclature, till I have got some body of information which I can communicate by means of them. It is only in that way that systems of names and notes make themselves generally known and used.

The personal names that have become attached to certain objects or concepts—Ohm's law, Rydberg's constant, Poynting's vector, Addison's disease—are usually mere labels for identification; we no more think of the persons connected with them than we ordinarily do of the historical characters who give their names to our streets. It is true that something of the history of science can be traced through such names, but it would require a good deal of research, and be only a partial history, since many of them are obscure, and many great names are not attached to particular discoveries. In practice they survive, as other terms do, merely for their convenience. I am informed (*14*) that there is a present tendency in medicine to replace the personal names of diseases by new terms that give a clearer idea of what the diseases really are. Ideally, a name should be both significant and convenient; but if it once gets into use, convenience is likely to carry the day, as the numerous clipped or hybrid forms that we have encountered show.

These various illustrations suggest, as is indeed the case, that new scientific terms are predominantly specific nouns or verbal forms derived from them by suffixes such as *-ize* and *-ation*. More general nouns seem to be formed in much the same way as specific ones; thus, in the 1820's, Dutrochet coined *endosmose* and *exosmose* (from a rather rare Greek word meaning "thrust") for the phenomena shown by permeable membranes. They were promptly adopted in England, both in the French forms and in those ending in *-osis*, which seem more in accordance with English usage, although both types have continued in use. Again, a term may be found serviceable even if its meaning is not entirely precise, as we see in the chemical notion of valency. Earlier concepts have had to be modified in the light of the electronic structure of the atom, yet they are still workable enough in the consideration of many of the simpler chemical phenomena, and a more precise formulation would in such cases be more trouble than it is worth. W. G. Penney says (*15*):

> If it were possible to work out in practice, instead of only in principle, the interactions and the energy of any system of atoms, the need for introducing the concept of valency would never arise.

Historically, however, matters did not proceed in this way, and utility is even now sufficient justification for the concept and the term.

The mention of osmosis suggests another aspect of terminology, the existence of doublets and parallel forms. Alongside *ōsmos* is the unrelated *ŏsme*, "odor," which gives osmium its name: *allyl* derives its first element, not from the Greek *allos*, "other," but from the Latin *allium*, "garlic." Since the number of pronounceable syllables in any language is limited, such doublets of different origin are unavoidable. On the other hand, parallel Greek and Latin forms give rise to words of similar but differentiated meanings; thus, *ge* and *terra* for "earth" have several derivatives, as have *helios* and *sol* for "sun," but *selene* for "moon" has yielded far fewer than *luna*. Again, in many words *sol-* is related to *solus*, and even to *solvere*, producing more doublets of different meaning that have to be distinguished. In these respects scientific words have grown up as casually as any others and exhibit the same anomalies and inconsistencies. It must be left to the specific case to decide whether the paired forms are practically interchangeable synonyms or whether they express useful shades of meaning; but the mere fact that one has not crowded out the other suggests that some purpose is served by both.

The total vocabulary of science, then, is made up of the separate vocabularies of the several sciences, which have grown at times when new discoveries demanded new names, at a rate that seems peculiar to each case. In the more observational

sciences, the more attentive examination of objects reveals new aspects that seem to demand specific names; if other workers see the same need, the new terms win more or less acceptance; if not, they fall into disuse. Thus, in botany, Joachim Jung, although he published nothing in his lifetime, introduced a terminology that was diffused among others in the 17th century and initiated a new approach to systematic botany (*16*). Later the study of tissues and cells, aided by new microscopic techniques, brought to light a whole series of new facts for which names had to be provided. When one branch of science comes into contact with another, a fresh range of possibilities may emerge, and a new complex terminology may be built up. Today physics has thus spread in many directions, and we have astrophysics, geophysics, and biophysics, to name no others. The use of artificial radioisotopes to combat disease and to trace the minute processes of growth is only one conspicuous example out of many.

A final example may be taken from geology. Many names of minerals go back to antiquity, others are given from their discoverers; many formations are named from the districts where they occur, often in casual and haphazard fashion, and descriptive terms have multiplied to an often embarrassing extent (*17*). Thus the Karst region of Dalmatia has provided a generic term for its type of limetsone formation: but, as a recent study (*18*) notes, its features are so distinctive

> . . . that there have grown up parallel elaborate systems of nomenclature, deriving from local vernaculars, for the description of land-forms, etc. As a result, British, French, and German writings on the subject are exasperatingly studded with Serbian and other words which have taken on the status of technical terms. . . . It is interesting that the parallel local systems exist, but it should be unnecessary to burden writings in any one language with a polylingual jargon of ugly and ambiguous terms.

How far this condition can be corrected, however, remains to be seen, since, as we have noted, it is often easier to establish a terminology than to improve it.

Thus the vocabulary of science is revealed as a vast and varied store of words gathered from many sources but with a preponderance of terms derived from Greek and Latin and diffused, by the spread of scientific activity, into most of the modern Western languages. Many of them are practically identical in the different tongues, but others, especially if they go back for a considerable period, are more or less modified in accordance with the characteristic sounds or endings of a particular language. The existence of this body of related words has sometimes been said to constitute the basis of a proposed international language, but the claim is a shaky one. As we have seen, many of the terms in question are nouns, and no language can get along with nouns exclusively. Let us admit, for instance, that *zinc* has been an "international" term for some 300 years; but suppose we want to say something about zinc—where the best ores occur, how they may best be treated, or any number of similar questions. The mere fact that the word *zinc* is generally known does not carry us very far, and there are no international terms for gold and iron. The truth is that the supposed international vocabulary is the product of historical accident, not of conscious and careful choice.

The case for the latest proposal in this direction was recently presented in these pages (*19*), and I need here point out only one weak spot in it. Alexander Gode declares that the little sentence-building words of English are rarely replaceable by Romance doublets, but that on the whole they perform comparable functions and correspond fairly well to Romance words. That is, they correspond when their functions are similar; but how should they do otherwise? But inspection of the Interlingua dictionary reveals that the words that serve these connective and adverbial functions are in most cases taken directly from Latin, for the simple reason that they are not uniformly represented in the modern languages from which "Standard Average European" is supposed to be extracted. These are the words that largely make connected discourse possible; and if they remain in their Latin forms, why go through the laborious process of devising for other words "average" forms which in most cases look merely like Latin gone more or less wrong? This boiling-down process has never yet been found acceptable on a large scale, although attempts have often been made; nor is it clear that there is any strong demand for it on the part of working scientists.

If we give up the search for a professedly new language, is there still room for improvement in the languages we now have? Certainly there is; but it is more likely to be brought about by small changes than by sweeping ones. Faraday's single-handed installation of terms is much less typical than the casual confusion that attended the naming of the new particles, and there seems to be no governing machinery by which the adoption of a term can be either encouraged or discouraged. In 1904 Charles Sanders Peirce wrote to William James (*20*):

> It is an indispensable requisite of science that it should have a recognized technical vocabulary, composed of words so unattractive that loose thinkers are not tempted to use them. . . . I wish you would reflect seriously upon the moral aspect of terminology.

Surely unattractiveness is not a necessary attribute of morality; and Peirce can hardly have believed that scientific terms can be restricted to a charmed circle of initiates. Anyone can use a word if he wants to, subject to the peril of misusing it if he does not wholly know what he is talking about; but in the world as it is the words of science must take their chances with the rest.

So far as individual words are concerned, they prove their fitness by surviving, and if they do survive, little more can be said. They may be unattractive, they may be illogical, but if they make their way, neither taste nor logic can furnish much opposition. *Antibiotic* is neither very attractive nor very clear, but it seems to be well established. Would anyone seriously urge replacing *hydrogen* by *protium* because the latter is a logical mate for *deuterium*? It would be much if we could exclude words that are forbiddingly impersonal, the extreme hybrids and abbreviations which a recent writer (*21*) has aptly called "shadowy and inhuman devices appearing and disappearing in that strange border country which separates speech from the mathematical sciences." Where possible, technical terms should not be too closely crowded in any passage; they need a little air around them, unless their sole purpose is to convey information as impersonally as possible—a purpose that can be overdone. The most severely technical words have to consort with those of a less austere sort, and the judicious writer should pay some heed to making them get along amicably.

After all, there is no reason to suppose that to talk or write about the things that scientific terms denote is essentially different from talking or writing about anything else. Certain aspects of the material may require special care in presentation, and no doubt certain concepts are not to be understood at first sight; but there are plenty of well-written scientific books, and if there are dull scientific articles, there are plenty of dull novels too. There is no virtue in excessive complication, but equally there is none in trying to make complicated matters appear simpler than they are. Each topic must be approached on its own merits, and both writer and reader must be disposed to give it a fair chance. The literary problem here is not different from what it is in other fields, and nothing is gained by supposing that science occupies a peculiar position. It does not require a language of its own, but should take its place alongside the other branches of human endeavor, and work out its destiny by the same means that they command.

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No one can be really master of any science unless he studies its special history, which again is bound up with the general history of humanity.—A. COMTE.

ASSOCIATION AFFAIRS

INTERNATIONAL ARID LANDS MEETINGS IN NEW MEXICO

A major effort to advance international cooperation of scientists from many fields in study of the future of arid lands will be made in New Mexico this spring. The American Association for the Advancement of Science is sponsoring an *International Arid Lands Symposium and Conference* in three parts. The symposium will be held at the University of New Mexico in Albuquerque on 26–29 April, a field trip in the Rio Grande Valley between Albuquerque and El Paso will take place on 30 April and 1 May, and a conference for a smaller group will be held at the New Mexico Institute of Mining and Technology in Socorro on 2–4 May.

The idea for such a gathering came from the Committee on Desert and Arid Zone Research of the Southwestern and Rocky Mountain Division of the association. It was approved last summer by the national AAAS board of directors which then sought support for carrying out the project. Helpful grants toward the cost of the symposium and conference since then have been received from the National Science Foundation and the Rockefeller Foundation. UNESCO has granted funds toward the cost of travel of foreign participants and has convened a meeting of its Advisory Committee on Arid Zone Research in the same region at that time so that members of the committee may take part.

Discussions planned for the symposium and conference will build upon several international arid zone gatherings of the past 5 years. A broad view of desert problems was developed at the Israel conference in 1952 [*Desert Research,* Research Council of Israel Special Publication No. 2 (1953)]. Hydrologic questions received primary attention at the Ankara symposium [*Proceedings of the Ankara Symposium on Arid Zone Hydrology,* UNESCO (1953)]. Biologic aspects of deserts, both hot and cold, were considered at London [*Proceedings of the Symposium on the biology and productivity of hot and cold deserts,* Institute of Biology (1954)]. Plant ecology was the central interest of the Montpellier Symposium in 1953 [report now in press]. Solar energy and wind power were the major topics in the symposium held at New Delhi last autumn [F. Daniels, *Science* **121,** 121 (28 Jan. 1955)].

The New Mexico meetings will differ from the previous ones in several important ways. They will concentrate attention on the frontiers of knowledge over a wide range of interrelated fields. There will be a small number of papers—all invited—and substantial time for discussion following them. Scientists from different fields will address themselves to the same problem. The final day of the symposium will be devoted to discussion in small groups dealing with problems that cut across several disciplines. Since the conference must be limited in numbers so as to promote fruitful exchange of ideas, participation will be by invitation only. The participants will focus attention on a few lines of investigation that seem, in the light of the symposium, to promise benefits from international or interdisciplinary collaboration. Rather than summarize the results of past work, the discussion will, it is hoped, concentrate upon new directions for research.

The future utility of the resource base for large segments of the arid zone around the world is far from secure. If their occupancy is to be maintained or extended, the exchange and application of present information must be accelerated and new understandings must be won. The fact that the meetings will be held in a region whose future seems closely linked with the solution of troublesome questions of salinity, silting, and land use should add weight to this emphasis.

It is estimated that scientists from more than 25 countries will take part. The range of the problems to be considered and of the experience of those invited to give papers is shown in the following preliminary program.

See advertising pages of this issue for registration and housing information. For copies of the program or other information, write American Association for the Advancement of Science, 1515 Massachusetts Avenue, NW, Washington 25, D.C., U.S.A. Cable: Advancesci Washington.

GILBERT F. WHITE
Haverford College, Haverford, Pennsylvania
PETER C. DUISBERG
Desert Products Company and *Allied Laboratories, El Paso, Texas*

Symposium: What Is the Future of Arid Lands?

26 APRIL

Evening lecture, 8 P.M. (open to the public). *Chairman,* George W. Beadle, department of biology, California Institute of Technology, Pasadena, and president of the AAAS, welcome for the Association; The Honorable John Simms, Governor of New Mexico, welcome for the Southwest; Tom J. Popejoy, president of the University of New Mexico, welcome for the University; Joe Dennis, department of chemistry, Texas Technological College, Lubbock, and president of the Southwestern and Rocky Mountain Division of the AAAS, welcome for the Division. Address by Homer L. Shantz, Santa Barbara, Calif., "History of arid zone development and the problems and potentialities of arid lands."

27 APRIL

Technical session I, variability and predictability of water supply in arid regions. Questions: How predictable is precipitation in an arid region? Are there distinct drought cycles? What are the prospects for usable ground water occurrence in arid areas? What is the practicability of locating and estimating volume and rate of natural recharge of underground water supplies? Within a given watershed, to what degree can the water sources and water yield be determined? *Chairman,*

Reed W. Bailey, director, Intermountain Forest and Range Experiment Station, U.S. Department of Agriculture, Ogden, Utah.

9 A.M. C. W. Thornthwaite, director, Johns Hopkins Laboratory of Climatology, Seabrook, N. J.

9:30 A.M. Jean Tixeront, ingenieur en chef des Travaux Publics, Tunis.

10 A.M. Luna Leopold, Water Resources Division, Geological Survey, U.S. Department of the Interior, Washington, D.C.

10:10 A.M. F. Dixey, director of Colonial Geological Surveys, Imperial Institute, London.

10:20 A.M. C. C. Wallén, assistant director, Swedish Meteorological and Hydrological Institute, Stockholm.

10:40 A. M. John H. Dorroh, Jr., hydrologist, Soil Conservation Service, U.S. Department of Agriculture, Albuquerque.

10:50 A.M. Terah L. Smiley, geochronologist, Laboratory of Tree-Ring Research, University of Arizona, Tucson.

11 A.M. Discussion.

11:45 A.M. Summary by the chairman.

Technical session II, Better use of present resources. Questions: What are the possibilities of increasing and maintaining sustained production from grass and forest lands without accelerating erosion? What are the consequences of utilizing arid lands beyond their capabilities? What constitutes wise allocation of available water supplies among the various needs in arid land drainage areas? How can production be increased from existing water supplies? Can irrigated lands be occupied permanently? *Chairman*: Kanwar Sain, chairman, Central Water and Power Commission, Ministry of Irrigation and Power, New Delhi, India.

2 P.M. R. O. Whyte, agronomist, Agriculture Division, Food and Agriculture Organization of the United Nations, Rome.

2:30 P.M. L. N. McClellan, assistant commissioner and chief engineer, Bureau of Reclamation, U.S. Department of the Interior, Denver, Colo.

3 P.M. Hilgard O'Reilly Sternberg, Centro de Pesquisas de Geografia do Brasil, faculdade nacional de filosofia, Universidade do Brasil, Rio de Janeiro.

3:10 P.M. L. A. Richards, physicist, Salinity Laboratory, Agricultural Research Service, U.S. Department of Agriculture, Riverside, Calif.

3:20 P.M. Cyril Luker, field representative, Soil Conservation Service, U.S. Department of Agriculture, Washington, D.C.

3:40 P.M. Raymond Price, director, Rocky Mountain Forest and Range Experiment Station, Forest Service, U.S. Department of Agriculture, Fort Collins, Colo.

3:50 P.M. Pedro Armillas, archeologist, Instituto Nacionale de Anthropologia, Mexico City.

4 P.M. Discussion.

4:45 P.M. Summary by the chairman.

Evening lecture, 8 P.M. (open to the public). *Chairman*, Peter C. Duisberg, Southwestern Irrigated Cotton Growers Association and Desert Products Company, and chairman, Committee on Desert and Arid Zone Research of the Southwestern and Rocky Mountain Division, AAAS. Address by Charles E. Kellogg, assistant administrator for soil survey, Soil Conservation Service, U.S. Department of Agriculture, Washington, D.C., "Current status of arid zone research and development."

28 APRIL

Technical session III, Prospects for additional water sources. Questions: How practicable is it to induce precipitation? How practicable is it to demineralize saline water? How practicable is it to reuse waste waters? What are the social and economic implications of these programs? *Chairman,* E. J. Workman, president, New Mexico Institute of Mining and Technology, Socorro.

9 A.M. Edward G. Bowen, chief, Division of Radiophysics, Commonwealth Scientific and Industrial Research Organization, Sydney, Australia.

9:30 A.M. Sheppard T. Powell, consulting engineer, Baltimore, Md.

10 A.M. W. F. J. M. Krul, professor at the University of Delft, and director, Government Institute of Water Supply, The Hague, Netherlands.

10:10 A.M. Vincent J. Schaefer, director of research, The Munitalp Foundation, Schenectady, N. Y.

10:20 A.M. Glenn W. Brier, chief, Meteorological Statistics Section, U.S. Department of Commerce, Washington, D.C.

10:40 A.M. H. E. Hayward, director, Salinity Laboratory, Agricultural Research Service, U.S. Department Agriculture, Riverside, Calif.

10:50 A.M. Louis Koenig, associate director, Southwest Research Institute, San Antonio, Tex.

11 A.M. Discussion.

11:45 A.M. Summary by the chairman.

Technical session IV, Better adaptation of plants and animals to arid conditions. Questions: What screening procedures would lead to the selection of more productive plant and animal species for arid regions? What are the genetic and physiological bases for drought resistance in plants and animals? What are the prospects of increasing drought resistance through genetic research? How can we develop a program of revegetation? What are the economic possibilities in the development and utilization of arid land plants and animals? What are the possibilities of maintaining larger human populations in arid areas? *Chairman,* Olaf S. Aamodt, technical specialist, plant sciences, Agricultural Research Service, U.S. Department of Agriculture, Beltsville, Md.

2 P.M. Colonel Omar Draz, Egyptian Army Veterinary Corps, director, Desert Range Development Project, Desert Institute, Heliopolis, Egypt.

2:30 P.M. R. Merton Love, professor of agronomy, Agricultural Experiment Station, University of California, Davis.

3 P.M. Knut Schmidt-Nielsen, professor of zoology, Duke University, Durham, N. C.

3:10 P.M. Enrique Beltrán, director, Instituto Mexicano de Recursos Naturales Renovables, Mexico City.

3:20 P.M. B. P. Uvarov, director, Anti-Locust Research Centre, London.

3:40 P.M. Michael Evenari, professor of botany and vice president, The Hebrew University, Jerusalem.

3:50 P.M. L. M. Pultz, principal horticulturist, Agricultural Research Service, U.S. Department of Agriculture, Beltsville, Md.

4 P.M. Discussion.

4:45 P.M. Summary by the chairman.

Evening lecture, 8 P.M. (open to the public). *Chairman,* Gilbert F. White, president, Haverford College, chairman, AAAS Planning Committee for the meetings, and U.S. member, UNESCO Advisory Committee on Arid Zone Research. Address by B. T. Dickson, botanist, retired chief, Division of Plant Industry, Commonwealth Scientific and Industrial Research Organization, Canberra, Australia, "The challenge of arid land research and development for the benefit of mankind."

29 APRIL

Discussion groups.

1) "Possibilities for drought prediction in arid areas," *chairman,* Erik K. Reed, regional chief of interpretation, National Park Service, U.S. Department of the Interior, Santa Fé, N. M.

2) "Possibilities for weather modification and control in arid areas," *chairman,* S. E. Reynolds, Research and Development Division, New Mexico Institute of Mining and Technology, Socorro.

3) "Prospects for improved estimation of underground water resources and recharge rates," *chairman,* R. W. Sundstrom, district engineer, Ground Water Branch, Geological Survey, U.S. Department of the Interior, Austin, Tex.

4) "Irrigation and industrial possibilities of desalination and planned use of saline water," *chairman,* T. F. Buehrer, head, department of agricultural chemistry, College of Agriculture, and Agricultural Experiment Station, University of Arizona, Tucson.

5) "Possibilities for water conservation and maintenance of favorable salt balances on irrigation projects," *chairman,* M. R. Huberty, head, department of irrigation and soils, University of California, Los Angeles.

6) "Prospects for grassland range improvement and conservation in arid areas," *chairman,* K. S. Valentine, associate professor of animal husbandry, New Mexico College of Agriculture and Mechanic Arts, State College.

7) "Potential relationships between soil conservation practices and watershed yield in arid areas," *chairman,* Robert B. Hickok, supervisor, Southwest Watershed Studies, Agricultural Research Service, U.S. Department of Agriculture, Albuquerque.

8) "Importance of wildlife conservation in arid areas," *chairman,* Walter P. Taylor, visiting lecturer, The Claremont Graduate School, Claremont, Calif.

9) "Possibilities for development of drought-resistant plants for arid areas," *chairman,* Lora M. Shields, department of biology, New Mexico Highlands University, Las Vegas.

10) "Possibilities for desert plant utilization in arid areas," *chairman,* E. F. Castetter, dean, graduate school, and chairman, department of biology, University of New Mexico, Albuquerque.

11) "Possibilities for animal improvement and utilization in arid areas," *chairman,* H. L. Stahnke, head, Department of Biological Science, Arizona State College, Tempe.

12) "Administrative and legal problems of arid land development," *chairman,* R. H. Walker, dean and director, Agricultural Experiment Station, School of Agriculture, Utah State Agricultural College, Logan.

13) Special symposium and discussion group. "Problems of the upper Rio Grande—a typical arid zone watershed," *chairman and program,* to be announced.

30 APRIL–1 MAY

Field Trip, the field trip (limited to the first 222 who apply) will leave Albuquerque the morning of 30 April and will proceed to El Paso, Tex., by way of the Estancia Valley and White Sands. Major stops will be made at White Sands, the malpais area west of Carrizozo, and other points. During the evening in El Paso, John H. Leasure, chairman, National Convention of the Cactus and Succulent Society of America, will show colored slides of cacti for those who are interested. On the return from El Paso to Albuquerque on 1 May, the trip will go up the Rio Grande Valley with stops at a large irrigated pecan orchard, a cotton farm, Elephant Butte Dam, and other locations. (Further details about final itinerary and arrangements may be obtained from Dr. J. Linton Gardner, P.O. Box 127, State College, N. Mex., U.S.A.)

2–4 MAY

Conference, (attendance by invitation only) Host institution, New Mexico Institute of Mining and Technology, Socorro.

ASSOCIATION BUSINESS

The 1954 meeting of the Council was held in two sessions on 27 and 30 December, on the campus of the University of California at Berkeley. President Warren Weaver presided.

Elections and Officers

The Council elected Paul B. Sears as President Elect, and Chauncey D. Leake and Margaret Mead as members of the Board of Directors. The entire list of general officers of the Association (those elected by the Council), including those elected at earlier meetings, is as follows. (Expiration dates are given in parentheses following the names of officers who serve for fixed terms of more than one year.)

Retiring President and Chairman of the Board of Directors: Warren Weaver (1955), Rockefeller Foundation

President: George W. Beadle (1956), California Institute of Technology

President elect: Paul B. Sears (1957), Yale University

Other members of the Board of Directors are John R. Dunning (1955), Columbia University; Laurence H. Snyder (1955), University of Oklahoma; Mark H. Ingraham (1956), University of Wisconsin; Paul E.

Klopsteg (1956), Glenview, Illinois; Wallace R. Brode (1957), National Bureau of Standards; Thomas Park (1957), University of Chicago; Chauncey D. Leake (1958), University of Texas Medical Branch, Galveston; Margaret Mead (1958), American Museum of Natural History; Paul A. Scherer (ex officio), Carnegie Institution of Washington; and Dael Wolfle (ex officio), AAAS.

Vice Presidents and Chairmen of the Sections for 1955 are (A) Mathematics, H. F. Bohnenblust, California Institute of Technology; (B) Physics, Alan T. Waterman, National Science Foundation; (C) Chemistry, Robert S. Ingols, Georgia Institute of Technology; (D) Astronomy, Seth B. Nicholson, Mount Wilson Observatory; (E) Geology and Geography, Carl Tolman, Washington University; (F) Zoological Sciences, J. Gordon Carlson, University of Tennessee; (G) Botanical Sciences, Lawrence R. Blinks, National Science Foundation; (H) Anthropology, W. Montague Cobb, Howard University; (I) Psychology, S. S. Stevens, Harvard University; (K) Social and Economic Sciences, Conrad Taeuber, Bureau of the Census; (L) History and Philosophy of Science, Raymond J. Seeger, National Science Foundation; (M) Engineering, Clarence E. Davies, American Society of Mechanical Engineers; (N) Medicine, not yet elected; (Nd) Dentistry, H. Trendley Dean, American Dental Association; (Np) Pharmacy, Glenn L. Jenkins, Purdue University; (O) Agriculture, John Fielding Reed, American Potash Institute; (P) Industrial Science, not yet elected; (Q) Education, Carter V. Good, University of Cincinnati.

New-section secretaries appointed by the Board of Directors are (K) Social and Economic Sciences, Donald P. Ray (1958), George Washington University, Washington, D.C., and (Np) Pharmacy, John E. Christian (1958), Purdue University, Lafayette, Ind.

Affiliated and Associated Societies

Upon recommendation by the Committee on Affiliation and Association and the Board of Directors, the Council voted to elect nine new affiliates, to change one associated society to an affiliated society, and to elect one new associate. New affiliates are American College of Chest Physicians, American Ornithologists' Union, American Physical Therapy Association, Association of Geology Teachers, Endocrine Society, Instrument Society of America, National Association for Research in Science Teaching, Radiological Society of North America, Inc., and Scientific Research Society of America.

The American Dairy Science Association was changed from associate to affiliate status.

The new associate is the National Association of Boards of Pharmacy.

At the end of the year there were 163 affiliated societies, 42 affiliated academies of science, and 53 associated societies, making a total of 258.

Changes in Bylaws

Changes in the Association's bylaws may be made by the Board of Directors but are subject to approval by the Council. The Council approved the following changes. (New material is in italics.)

Article VI, Section 3: The Southwestern *and Rocky Mountain* Division (organized in 1920) includes members of the Association resident in Arizona, New Mexico, Colorado, Wyoming, Montana east of the Continental Divide, Sonora, Chihuahua, and Texas west of the 100th meridian.

Article VI, Section 4: The Alaska Division (organized in 1951) includes members of the Association resident in Alaska *and others who meet such requirements as may be established by the Division and approved by the Board of Directors.*

Article VI, Section 5: Each division shall receive for its expenses an annual allowance not to exceed one dollar for each of its members *who is an AAAS member* in good standing, *or three hundred dollars ($300) whichever is larger,* and shall make an annual report to the Board of Directors covering its financial situation and other activities.

At the time of the 1953 meeting, the constitution and bylaws under which the Association operates had been in effect for 1 year. In order to secure a general review and in order to correct some difficulties, a special committee on Constitution, Bylaws, and General Operations was authorized at the 1953 meeting. The chairman, Wallace R. Brode, presented a preliminary report in which he described the nature of the changes which the committee had considered and asked for advice and recommendations from the Council. The discussion led to agreement that the committee would propose specific changes for action in 1955.

The text of several resolutions passed is given in the issue of *Science* for 18 February.

The National Citizens Committee for Educational Television presented the Association with a citation for its assistance in advancing the cause of educational television. On behalf of the Association, the citation was received by John A. Behnke, Associate Administrative Secretary.

Council discussed at some length possible alternative methods of electing the Association's president elect. Consideration was given to the question of whether election should continue to be by the Council or should be broadened so that all fellows, or perhaps even all members, would have the privilege of voting. Also discussed were methods of securing more extended consideration of the candidates from among whom the Council would select a president elect, as were methods of securing votes from a larger fraction of the Council than is usually present at an annual meeting. After discussion of several specific proposals, none of which came to vote, it was agreed that the problem should be considered by the Committee on Constitution, Bylaws, and General Operations, and that at the 1955 meeting the Board of Directors should submit one or two specific proposals for Council consideration.

The chairman of the Association's Building Committee, John R. Dunning, described the headquarters building that the Association plans to erect and indicated that construction would start about 1 April 1955. In a discussion of the financing of the new building it was pointed out that most of the cost could be met

from the surplus of income over expenses in prior years.

The chairman of the Publications Committee, Paul B. Sears, reported that Duane Roller, who served as editor of *Science* and the *Scientific Monthly* since December 1953, had resigned and that, pending the completion of more permanent editorial arrangements, the Board of Directors had asked the Administrative Secretary to serve as Acting Editor.

DAEL WOLFLE
Administrative Secretary

REPORT OF THE BERKELEY MEETING, 26–31 DECEMBER 1954

The 121st meeting of the American Association for the Advancement of Science—unique in that it was the Association's first national winter meeting west of the Rockies and also the first time in many decades that the AAAS has been able to meet, almost exclusively, upon a campus—was a truly memorable occasion. Although, inevitably, the housing accommodations were more scattered and less convenient than can be offered by the large hotels of a great city, this was offset by economies in cost, and the meeting was a decided success in all essential respects. It will long be remembered as one of the most pleasant, fruitful, and significant in the history of the AAAS.

Not only was this the largest diversified scientific meeting ever held on the Pacific Coast but it was one of the largest meetings ever sponsored by the Association, now in its 107th year. There were 3856 paid AAAS registrants, more than in any year since the exceptional 1949 meeting in New York. The Association does not make registration mandatory so it is probable that at least 4500 scientists, science teachers, and other science-minded persons were in attendance the week of 26–31 Dec. An *additional* 3612 persons visited the exhibits.

In rich variety and high quality, the programs of the Berkeley meeting were impressive. No principal field of science was neglected. There were programs for specialists arranged by large societies in these fields and by some of the Association's sections. Characteristic of AAAS meetings, other sections and many of the participating societies organized strong interdisciplinary symposiums which brought diverse specialists together to consider areas of common concern. Of interest to nearly all scientists, such important matters as air pollution, scientific editorial problems, scientific manpower, the shortage of science teachers, and national security were presented to appreciative audiences. There were also all the customary features expected at AAAS meetings—outstanding evening addresses, the latest scientific films, large-scale exhibits, and, on the last evening, the traditional Biologists' Smoker with refreshments.

Despite the record-breaking number of 105 symposiums, groups of invited papers, and panels, apparently all programs had audiences satisfactory to their chairmen. Indeed, there were numerous instances of expectations exceeded, which in some cases necessitated last-minute changes to a larger session room. In addition to the programs of the AAAS sections, an unprecedented number of 90 societies and other organizations participated, 57 with programs of their own, the others as official cosponsors. One result was still another record: the AAAS General Program-Directory reached the unprecedented size of a 17-oz book of 420 pages.

Among the memorable events of the Berkeley meeting were the AAAS presidential address and reception, 28 Dec. President Robert Gordon Sproul's remarks when, as host and general chairman, he welcomed the AAAS to the campus, were delightful in their warmth and humor. Following President Warren Weaver's introduction of the retiring president, E. U. Condon received a spontaneous standing ovation prior to delivering his address, "A half century of quantum physics." Not only was the auditorium of Wheeler Hall filled to capacity with some persons standing, but two additional rooms equipped with public address systems were also filled beyond normal capacity. The AAAS reception that followed in nearby Stephens Memorial Union was thoroughly enjoyable, with refreshments arranged by Mrs. Sproul. Representatives of the Chinese and South African associations for the advancement of science were among those in the receiving line. The previous afternoon, Mrs. Sproul and Mrs. Clark Kerr had entertained some 400 ladies attending the meetings at tea in the Women's Faculty Club.

A significant occasion was the all-afternoon program for 850 selected high school students from the San Francisco Bay area interested in science as a career, sponsored by the Association's Academy Conference and held on the new campus of San Francisco State College, a cosponsor. On 28 Dec. this audience of scientists of tomorrow enjoyed addresses and expert counsel on opportunities and careers in science from Warren Weaver and Harry C. Kelly, the GE "House of Magic," a panel, "Youth looks at science," moderated by James Stoney, and special film showings of *Nature's Half Acre* and *Water Birds,* through the courtesy of Walt Disney Productions. Local chairmen in charge of the entire project were Eugene Roberts, president of the California Science Teachers Association, northern section, and Robert Stollberg, professor of physical science and education at San Francisco State College. This Ninth Junior Scientists Assembly, especially arranged, was a departure from previous programs and marked the intention of the Association to do more for precollege science students at its annual meetings.

AAAS general symposium. In Feb. 1954, the AAAS Symposium Committee, consisting of 19 prominent West Coast scientists, Warren Weaver, and the writer, ex officio, met in Berkeley to plan the General Symposium of the 121st meeting. One three-session general symposium, *Science and Society,* was decided upon, and separate program chairmen were chosen to organize each part, scheduled for 4 P.M., 27–29 Dec., inclusive. Part I, "Natural resources," arranged by Louis B. Slichter, speakers, Farrington Daniels, Evan Just (in

absentia and J. G. Harrar; part II, "Population problems," arranged by Curt Stern, speakers, Curt Stern, Dudley Kirk, and Alan Gregg; and part III, "Science in human thought and action," arranged by Roger R. Revelle, speakers, Joel Hildebrand, Bart Bok, and John von Neumann; all these sessions enjoyed appreciative audiences.

Other symposiums. At Berkeley, the sections and societies together scheduled 105 symposiums, groups of invited papers, panels, or other sessions centered about a particular theme. The number is larger than that for any previous AAAS meeting—the previous high was 62 at St. Louis. Of the total 368 sessions of all types, the Association and its sections arranged 52 sessions for symposiums (217 papers) and 33 for 216 contributed papers; 30 of the participating societies scheduled 85 sessions for symposiums (405 papers) and 64 for 518 contributed papers.

Conferences. There are at present three recurrent conferences at AAAS meetings. The Academy Conference made up of the official delegates of the 42 academies of science affiliated with the Association had a fruitful day of reports and round-table discussions, an afternoon address on the National Science Talent Search by Margaret Patterson, and a dinner with a presidential address by Wayne Taylor. The Conference on Scientific Manpower that met at Berkeley for the fourth time on an organized basis held two sessions. The third Conference on Scientific Editorial Problems increased its sessions to four.

Special sessions. The distinguished evening addresses that add so much to meetings of the Association each year were continued at Berkeley and, without exception, all had large and responsive audiences. In chronological order, the speaker for the National Geographic Society's annual lecture and color motion picture was W. Robert Moore; his subject, "Five-star Burma," was enjoyed by some 3400 persons in the Berkeley High School auditorium. The 15th annual address of the United Chapters of Phi Beta Kappa with the AAAS was delivered by Conway Zirkle; his subject, "Our splintered learning and the status of scientists," provocatively phrased, was an eloquent plea for a much broader and better integrated foundation in the liberal arts training of scientists. The AAAS presidential address, E. U. Condon's "A half century of quantum physics," not only was a scholarly review of developments in this field but was enlivened by personal anecdotes that revealed how the author's own interest began. The annual address of the Society of the Sigma Xi, the latest in a series that began in 1922, was given by one of the University of California's own scientists, Nobel prize winner Edwin M. McMillan, who spoke on "High-energy physics." The seventh annual address of the Scientific Research Society of America, "Science and progress?" delivered by Vannevar Bush, dealt with science and national affairs, especially the need for a better system of national security. An additional special session at Berkeley was an afternoon address, 30 Dec., arranged by the Pacific Science Board. Alexander Spoehr's subject, "The Human background of Pacific science," rounded out a brilliant series of public lectures at the Berkeley meeting.

Attendance. The total of 3856 paid registrations at Berkeley is some 4 times the registration totals of the June meetings of the AAAS Pacific Division. A geographic analysis of these registrants is given in Table 1.

An analysis of the subject fields of the registrants and of the additional visitors to the exposition is given in Table 2.

Physical arrangements. The Berkeley meeting enjoyed both exceptional physical facilities and the efficient services of many skilled persons. It is not an exaggeration that literally all the resources of the great University of California were placed at the disposal of the Association by the general chairman of the meeting, Robert Gordon Sproul. This meant not only classrooms, 58 of which were used for sessions, gymnasiums, residence halls, and cafeterias but also highly trained men and women at all levels.

The centrally located, large Gymnasium for Men was an admirable center for the entire meeting. The

Table 1. Distribution of registrants by states and countries.

Alabama	2	Texas	26
Arizona	26	Utah	48
Arkansas	3	Virginia	8
California	2904	Washington	100
Colorado	29	West Virginia	3
Connecticut	12	Wisconsin	25
Delaware	1	Wyoming	2
District of Columbia	39		
Florida	7	Total, continental U.S.	3786
Georgia	5		
Idaho	11		
Illinois	43	Alaska	3
Indiana	27	Australia	3
Iowa	18	Brazil	4
Kansas	9	Canada	23
Kentucky	3	Egypt	1
Louisiana	10	England	2
Maine	1	Formosa	2
Maryland	18	France	1
Massachusetts	28	Germany	1
Michigan	25	Hawaii	14
Minnesota	16	India	1
Mississippi	1	Israel	1
Missouri	8	Japan	1
Montana	9	Korea	2
Nebraska	9	Mexico	3
Nevada	21	Netherlands	1
New Jersey	11	Norway	1
New Mexico	21	Peru	1
New York	53	Philippines	1
North Carolina	9	South Africa	1
North Dakota	1	Spain	1
Ohio	22	Venezuela	2
Oklahoma	12		
Oregon	115	Total, territorial and foreign	70
Pennsylvania	31		
Rhode Island	5	Total registration	3856
Tennessee	9		

lobby contained the AAAS Registration and Information Centers and the main corridor next to it afforded space for a variety of tables for society registrations, room assignments in the residence halls, sale of tickets for meal functions, signing up for tours, and the like. The basketball floor, carefully covered with a heavy tarpaulin, was the locale of the Annual Exposition of Science and Industry. Large rooms near the exhibit housed a lounge, in which light refreshments were available, and the Visible Directory of Registrants and check room. The AAAS Science Theatre occupied a room at the other end of the central corridor. For this, excellent projection facilities were provided; and, typical of the thoughtfulness and thoroughness of the Committee on Physical Arrangements, those attending the theatre enjoyed comfortable leather-covered seats, which came from virtually every faculty office in the university.

Through the cooperation of the University Food Services, those who roomed in the residence halls enjoyed breakfasts "in" before leaving for sessions of their choice. Coffee and light refreshments were available during the day in the lounge of the Gymnasium for Men and in the attractive new Alumni Lounge, which served as a meeting place for all. The University Cafeterias, which served all meals and took care of simultaneous banquets, were a convenience much appreciated by the attendance.

Table 2. Attendance by subject fields.

Subject	3856 registrants	3612 complimentary admissions
Mathematics and statistics	197	81
Physical sciences		
Physics	258	394
Meteorology	32	6
Astronomy	75	54
Chemistry	345	287
Geology and geography	67	49
Engineering and industrial science	118	291
Biological sciences		
Plant pathology and mycology	68	18
Other botanical sciences	179	45
Ecology	60	
Limnology, oceanography, ichthyology	59	8
Genetics and cytology	64	10
Microbiology	51	7
Zoological sciences	374	107
Biology (in general, and other)	244	126
Agriculture and conservation	91	67
Medical sciences		
Bacteriology and virology	61	42
Biochemistry and nutrition	206	45
Biophysics	36	7
Physiology	108	18
Dental research	34	11
Pharmacy, pharmacology, toxicology	55	15
Medicine (in general, and other)	257	175
Psychology	156	67
Anthropology	43	25
Economic and social sciences	56	164
History and philosophy of science	18	32
Science teaching and education	209	293
Scientific editorial problems	43	22
General	292	794
No field indicated		352
Total	3856	3612

Housing facilities. Although, of necessity, every visitor could not enjoy a single room (relatively scarce at any convention), the hotels and motels of Berkeley, in conjunction with the university residence halls and International House, proved adequate housing accommodations for those who traveled to the meeting beyond commuting distance. An admirable job of processing the many applications for housing accommodations was done by the staff of the Convention Bureau of the Berkeley Chamber of Commerce, headed by Weldon E. Vance. His help in this and throughout the year is gratefully acknowledged.

AAAS public information service. The AAAS Press Room utilized the facilities of *The Daily Californian* in Eshleman Hall. Those who visited this busy room during the meeting gained some impression of the efficient way in which science writers and other reporters were provided with releases during the meeting by Sidney S. Negus, AAAS Director of Public Information. The Association is indebted to George C. Tenney of the McGraw-Hill Publishing Co., chairman of the local Committee on Public Relations; to Daniel M. Wilkes, science writer of the university; to Nancy Kern, who admirably handled radio and TV; and to many others for their invaluable assistance in helping to make possible the fulfillment of one of the principal objectives of the AAAS—to increase public understanding and appreciation of the importance and promise of the methods of science in human progress.

Reports and news of the Berkeley meeting in magazines will continue to appear during the year. One of the first of these was the first six pages of *Life* for 10 Jan. 1955.

AAAS Science Theatre. Beginning Monday morning, 27 Dec., and ending Thursday afternoon, 30 Dec., in eight programs, each 4 hours long, 47 of the latest foreign and domestic scientific films were presented. Each title was shown twice, with two exceptions of films shared with AAAS sections and for which other films were substituted.

Annual Exposition of Science and Industry. The 1954 Annual Exposition of Science and Industry with its 105 booths was a decided success in the opinion of the exhibitors, who are the final judges. For an hour prior to opening time, 2 P.M., 27 Dec., people had gathered before the entrance door. When at last it opened, people radiated through all the aisles, as one observer put it, "like logs filling a mill pond when a lumber drive is on." Thereafter, during the 4 days the exhibits were open, booth traffic was heavy and satisfactory. The visitors were seriously interested in the latest tools and techniques of science and industry, and the exhibitors were pleased with their response.

The names of most of the 66 exhibitors and descriptions of their exhibits appeared both in the General Program Directory and in the preconvention issue of *Science*. Those that did not are listed here:

Alameda Naval Air Station
California State Department of Public Health (sponsored by the Kaiser Steel Corp.)
Crown Zellerbach Corporation
Encyclopaedia Britannica, Inc.
Great Books of the Western World
Hazatrol, Inc.
Natcol Laboratories
University of California History of Science Library
University of California Press

As in past years, we are indebted to the Special Libraries Association, this time the San Francisco Chapter, for supervising the Science Library.

Work of the local committees. The officers and staff of the Association realize the magnitude of the exceptional services rendered the AAAS and the attendance by all the local committees. Sproul, who accepted the general chairmanship in 1953, appointed the several local committees early in 1954 and, with the assistance of the executive secretary, George A. Pettitt, kept in close touch with their work. It was obvious to those in attendance that all phases of the meeting were operating with unequalled smoothness. In a very large measure, this was due to the caliber of the Committee on Physical Arrangements and the degree of planning, supervision, and personal assistance all its members contributed. Particular mention is made of this committee's chairman, Richard H. Neddersen, acting business manager of the university, for he chose his fellow-members carefully, organized all details soundly, and, when the meeting began, virtually moved into the AAAS Office, where he solved a multitude of small vexations systematically and with expedition. He was ably assisted by Joseph W. Hutchison, principal superintendent of grounds and buildings, and J. E. Lateana, principal audio-visual specialist, who, respectively, supervised the supply of physical equipment and projection. Ruth N. Donnelly, housing supervisor, not only organized the use of the residence halls but assisted materially with the AAAS reception and other social aspects of the meeting. The employees of the university without exception were efficient, cooperative, and friendly to an unparalleled degree. The writer, the Association, and those who attended are especially indebted to those employees, faculty members and their wives, members and friends of the Association, and students who volunteered their services.

The splendid services of the Committee on Public Relations has been mentioned. The extent of the contribution of this committee was apparent to all who visited the press headquarters or read the newspapers of the Bay area.

The Committee on Exhibits, under the chairmanship of Robert K. Cutter, president of Cutter Laboratories, worked hard from March on to interest local industries in taking booth space. To this committee and Arthur K. Beckley of Cutter Laboratories, which absorbed all costs for local promotion of the exposition, goes the credit for securing representation from so many West Coast firms.

The Committee on Finance, with James B. Black, president of the Pacific Gas and Electric Co., as chairman, fully realized its objectives in raising funds to assist with the expenses of the meeting. The contributing firms were:

Allied Chemical and Dye Corp., General Chemical Division
American Forest Products Co.
American Trust Co.
Bakelite Co.
Bank of America
Bechtel Corp.
Bishop Oil Co.
Braun-Knecht-Heimann Co.
California and Hawaiian Sugar Refining Corp.
California Packing Corp.
Calveras Cement Co.
Carbide and Carbon Chemicals Co.
Columbia-Geneva Steel Division, U.S. Steel Corp.
Crown Zellerbach Corp.
Dean Witter and Co.
Dow Chemical Co.
Electro-Metallurgical Co.
Fibreboard Products, Inc.
Food Machinery and Chemical Corp.
General Electric Co.
General Metals Corp.
Haynes Stellite Co.
Henry J. Kaiser Co.
Hercules Powder Co.
Honolulu Oil Corp.
Ideal Cement Co.
Johns-Manville Corp.
Kaiser Aluminum and Chemical Corp.
Kaiser Engineers
Kaiser Steel Corp.
Kern County Land Co.
Levi Strauss and Co.
Linde Air Products Co.
Monsanto Chemical Co.
National Carbon Co.
Pabco Products, Inc.
Pacific Gas and Electric Co.
Pacific Telephone and Telegraph Co.
Permanente Cement Co.
Schlage Lock Co.
Shell Oil Co.
Spreckels Sugar Co.
Standard Oil Company of California
Stauffer Chemical Co.
Union Oil Company of California
Westinghouse Electric Corp.

To these should be added contributions made by the Hotel Claremont of Berkeley and by the United-Carr Fastener Corp. of Cambridge, Mass., the latter made

to the AAAS for any worthy purpose and applied to the Berkeley meeting. The Association gratefully acknowledges these contributions and expresses its deep appreciation to all members of the local committees.

Awards and Prize Winners at the Berkeley Meeting

AAAS 27th Newcomb Cleveland Prize of $1000—Daniel H. Alpert, research physicist, Westinghouse Electric Corp., for his paper, "Experiments at very low pressures," read before a joint session of the American Physical Society and Section B; 30 Dec.

AAAS 10th Theobald Smith Award in the Medical Sciences, given by Eli Lilly and Company—$1000, a bronze medal, and $150 travel expenses—Winston Harvey Price, associate professor of biochemistry, School of Hygiene and Public Health, Johns Hopkins University, for his epidemiological studies on Rocky Mountain spotted fever and typhus; presented in absentia at a session of Section N, 27 Dec.

Scientific Research Society of America—4th William Procter Prize of $1000—Vannevar Bush, president, Carnegie Institution of Washington, for scientific achievement and distinction in scientific administration; presented at the annual RESA address, 30 Dec.

Cranbrook Institute of Science—Mary Soper Pope medal—Irving W. Bailey, professor of plant anatomy, Harvard University, for distinguished work in botany; presented at the botanists' dinner sponsored by Section G and cosponsored by the American Society of Plant Physiologists, western section, the Botanical Society of America, Pacific section, and the Mycological Society of America.

John Scott Award—Robley C. Williams, research biophysicist, Virus Laboratory, University of California; presented at the end of the symposium on "Viruses" of Section C and the American Chemical Society, California section.

RAYMOND L. TAYLOR
Associate Administrative Secretary, AAAS

LETTERS

Time and Ethics

Adolf Grünbaum [*Sci. Monthly* **79,** 13 (1954)] has presented a reasonable argument for scientific naturalism, provided that we grant the validity of his premises. Of these, the limitations of space permit the discussion of only one, "the Deity itself must have been created at one time. . . ." Of his conclusions, I shall also consider only one, ". . . theism [is not] . . . logically necessary as an axiomatic basis for ethics."

Consider what time is. Time is the product of motion. Without motion there would be no time. What is motion? In a simple aspect it is change of position, say, from here to there. Formally, motion is the act of passing from the possible to the actual. Now I am here. If it is possible for me to go there, and I do so, I have moved. It is now not possible for me to go there because I now am there. (I can, of course, move from there and thereby render it possible for me to return there.) But let us say that I remain there; I moved from possibly being there to actually being there. This is motion, and as such it is concerned with any change from potency to act. If there is no motion, there is no time.

It involves no contradictions to postulate a Being to Whom, in this sense, nothing is possible because He is pure act; and to postulate that He is immutably in this state. Then for such a Being there would be no such thing as motion, no such thing as a creation of this Being at an instant in time.

If there need not be a moral (theological) basis for ethics, then there must be some good reason for adhering to a set of ethical principles. The promotion of the common good would, for example, be a reasonable basis. But surely I as an individual would properly have an interest in my own good. And surely if these two interests were in conflict I would be foolish not to put my own interests first. Even though I (and all the rest of the individual members of the human race) might not go to extremes in favoring my (their) own good, it is patent that we would soon reach a chaotic condition. Therefore it is necessary, for the common good, that we at least *postulate* an external ethical standard. And since it would necessarily be external to all men, it would have to be a theological standard.

Further, I submit that although conditions today might properly be called chaotic, they are in order to the exact degree that mankind, as a whole, practices the precepts of that set of ethical principles based upon the decalogue.

JAY A. YOUNG
Department of Chemistry,
King's College, Wilkes-Barre, Pennsylvania

J. A. Young's first objection ignores what we have learned concerning the relativity of motion since the time of Aristotle's mechanics and cosmology. Given that motion is *relational,* the entity which Young wishes to postulate must, by his own standards, be "immutable" with respect to *all* objects in the universe while most of the latter are constantly moving relatively to one another. Apart from this manifest contradiction, it should be noted that if his description of a being who is "immutably" in the alleged "state" of "pure act" is to have even a semblance of meaning, this description must denote an entity *actually* possessing *all* attributes whatsoever, including a host of mutually incompatible

ones. More fundamentally, however, I do not see how one can avoid the conclusion that "*pure* actuality" (and, correlatively, "*pure* potentiality") are meaningless terms that derive their *semblance* of meaning from contexts in which certain *specific* properties can be significantly said to be present or *actual* while certain other, *specific* properties are absent though *potentially* present. In short, even disregarding the false theory of motion ingredient in Young's first criticism, I submit that his account of the being which is "pure act" is just as unintelligible as, for example, the sentence, "Yesterday at 3:00 P.M., I laughed at a joke, wrapped my laugh in gift paper and mailed it to the irrational number $\sqrt{3}$."

The second objection rests on a logical blunder in which the distributive and collective senses of the term "all" are confused, as when Young speaks of "all men": granted that each one of us, taken individually, is to recognize the interests of the *corresponding* remainder of the community and thereby accept an ethical standard external to himself, it surely does *not* follow that the imperative to consider the common good would derive its sanction from a source external to the entire community of men, taken collectively. And even if Young had succeeded in showing that a theological standard is necessary, which he has not, nothing would have been gained unless he had *also* shown why one should accept his particular (Judaeo-Christian) standard rather than any one of the many other theological standards incompatible with his. Since he has ignored the various difficulties which I have urged against his ethical position on p. 18 of my article, I can only regard his espousal of the ethics of the Ten Commandments as a form of theological parochialism or cultural provincialism. Moreover, his letter fails to make clear how the decalogue can yield unambiguous answers to some of the crucial ethical perplexities of our time and can thereby provide a basis for social harmony under 20th century conditions.

ADOLF GRÜNBAUM

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Esperanto versus Interlingua

Alexander Gode [*Sci. Monthly* **79,** 65 (1954)] says that Interlingua is not a constructed language. Even though he would hate to admit it, Interlingua is as much a constructed language as Esperanto. Both of them are based on the two same main principles, an international vocabulary and a simplified grammar.

Being able myself to read English, German, French, Spanish, and Esperanto, I can also make some sense out of Interlingua. But his assumption that ". . . all speakers of a western language . . . read it at sight as a language familiar to them . . ." sounds too good. Having recently seen some examples of Interlingua, I am convinced that this is a gross exaggeration. Most people, reading Interlingua, would have to use a dictionary to be certain that they were getting the correct meaning. And if one has to use a dictionary, one might better use Esperanto in the first place, for its vocabulary is much simpler than Interlingua's.

The most important point I want to make is this: although Interlingua can, with difficulty, be read at sight, Esperanto can also, and just as easily.

The only way I can illustrate that point is to give an example. Lest it be said that I took an example that flatters Esperanto, I will take one used by the Interlingua people to demonstrate Interlingua. If anything, I am prejudicing the example in favor of Interlingua.

INTERLINGUA

Energia es necessari pro toto que occurre in le mundo. In tempores passate le plu grande parte del energia applicate esseva fornite per le fortia muscular del homines e del animales domestic.

Hodie carbon, petroleo, aqua, e ligno nos da le grosso del fortia motor. Vostre musculos e mies es exempte de labor nimis dur.

Fundamentalmente toto le energia de nostre terra veni del sol. Le calor e le lumine del sol- octo minutas distante de nos in tempore de viage del radiation- es cosa vital.

ESPERANTO

La energio estas necesa por ĉio, kio okazas en la mondo. En pasintaj tempoj, la plej granda parto de la aplikata energio proviziĝis per la muskoloj kaj laboro de homoj kaj servaj bestoj.

Hodiaŭ, karbo, petrolo, akvo kaj ligno donas al ni nian gravan forton. Via muskolo kaj mia estas liberaj de tro malfacila laboro.

Fundamente, la tuta energio de nia mondo venas de la suno. La varmo kaj lumo de la suno, ok minutojn for de ni laŭ la tempo de vojaĝo de la radio, estas afero necesa por vivo.

Not only can Esperanto be read at sight as easily as Interlingua, but it also has a much simpler vocabulary, is completely phonetic, and it has already become widespread. Many magazines and books are published in it, the Bible and *Kon-Tiki*, to mention only two. In addition, textbooks and dictionaries are available in many languages and Interlingua is just attempting to do a job that Esperanto can and is doing better. We need only *one* international language. Esperanto is the best. Why not use it?

RICHARD T. VINESKI

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Vineski makes three points: (i) Interlingua is as much a constructed language as Esperanto; (ii) Esperanto too can be read at sight; (iii) we need only *one* international language.

Interlingua is an attempt—useful but improvable by evolution—to codify the common norms underlying the major members of the cultural family of languages of the Occident. Its appearance is not influenced by subjective or logical or otherwise extraneous desiderata. Esperanto is a system, ideally rigid and practically im-

mutable, that operates with a limited number of root words selected by Zamenhof and an apparatus of logical structural devices which Zamenhof deemed useful and which he invented, or developed, or gathered from scattered existing languages. It is in general a matter of pride with Esperantists that they can claim that their language is the construction of one great man. Interlingua began pointing with pride to its not being constructed after this "weakness" had been attacked by dissenting Esperantists.

The comprehension test proposed by Vineski is unnecessarily unfavorable to Esperanto. The Interlingua passage was written in order to impress the reader, and with a conscious effort to avoid all the stumbling blocks that do exist in Interlingua. Furthermore, it is not quite fair to Esperanto to make so very clear that most of its difficulties get resolved after one has read the same passage in Interlingua.

I disagree violently and passionately with the idea that we need only one international language. It is impossible for free searchers after the better and the best to accept the claim that we need *one* solution for a given set of problems. Interlingua serves very nicely in certain fields where Esperanto cannot function. Let the Esperantists find the spheres where their language is service-wise superior to anything Interlingua can do. If they find them, Interlingua won't be able to do anything about it.

ALEXANDER GODE
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Determinism in Science

I have enjoyed reading the excellent articles on psychoanalysis that appear in the November issue of *The Scientific Monthly,* but in the article "On the scientific status of psychoanalysis," by Jerome Richfield, I find what seems to me a rather confused and confusing statement that is not supported by facts. ". . . the limitations set by Heisenberg's uncertainty relationship on any strict application of the principle of causality . . . that *science is no longer deterministic.*" [italics mine].

This, I submit, is grossly fallacious and misleading, for when science ceases being deterministic it will no longer be science and man will be unable to build a simple bridge. The indeterminism found in the moving electron in no way affects the macrocosm or the mass movements of things; otherwise it would be impossible to predict eclipses, the tides, chemical reactions, and other events. In other words, science would have lost its most important and fundamental quality—predictability.

I see no reason then for all this joy among certain starry-eyed philosophers and theologians because there is indeterminism in the electron, for this does not give us the right to believe in free will. It can be said that free will is an illusion that must be treated as a reality.

J. M. MARTINEZ
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I am inclined to believe that J. M. Martinez is somewhat overzealous in his desire to preserve his own philosophic notions about science. He did not appreciate that my article took no stand on the question of whether science was or was not deterministic. I was concerned with simply reporting that since there is widespread disagreement among philosophers and scientists over such questions, there could be no clear meaning to questions about the scientific status of some particular field. Martinez is somewhat prejudiced about "starry-eyed" philosophers; he will find many philosophers who agree with him, many who do not, and many who are interested in the question without being at all interested in establishing the possibility of the existence of free will.

I cannot feel free to speak for the theologians whom he thinks should be lumped together with philosophers, but as far as most of the latter are concerned, there is neither joy nor sorrow to be attached to the answers to purely philosophic questions. The joy is in philosophizing and not in the particular results.

Perhaps it should be suggested that Martinez' chief argument contains a *non sequitur.* It does not follow from the fact that man can build a bridge or predict an eclipse that science is deterministic. It is not enough that some events are predictable. We must be in a position to state that *in principle,* at least, all events are predictable.

There seems to be no justification for the gloom with which Martinez surrounds the position of indeterminism. Even if some events in quantum physics are in principle unpredictable, science will continue to be the important, useful activity it has always been.

JEROME RICHFIELD
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We publish this discussion because it brings out two points of view that are commonly confused by laymen and scientists alike. In this connection, readers are referred to Schroedinger's book, Science and Humanism, *in which this question is admirably discussed and literature references are given.—D.R.*

The Mesozoa

Since our article, "Strange life of the dicyemid mesozoans" went to press [*Sci. Monthly* **79,** 277 (Nov. 1954)], there has come to our attention, too late for inclusion in the list of references, an important recent article by Horace Stunkard of New York University entitled "The life-history and systematic relations of the Mesozoa" [*Quart. Rev. Biol.* **29**(3), 230 (Sept. 1954)]. In addition to reviewing previous work on the group, it presents some original work in the dicyemids and also proposes a definite taxonomic status for the Mesozoa.

BAYARD AND EVELYN MCCONNAUGHEY
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BOOK REVIEWS

The Inside Story: Psychiatry and Everyday Life. Fritz Redlich, June Bingham, and Jacob Levine. Knopf, New York, 1953. xiv + 280 pp. Illus. $4.

THIS "very funny book with a very serious purpose" was produced by the collaboration of Fritz Redlich, chairman of the department of psychiatry at Yale University School of Medicine, Jacob Levine, a clinical psychologist, and June Bingham, a free-lance writer who specializes in mental health subjects. It is written in ordinary everyday language by June Bingham. Appended is a glossary which defines technical terms in both the language of the psychiatrist and the layman. The result of this collaborative effort is a book that is amusing yet provocative, entertaining but instructive.

Just what is psychiatry and how is it a part of the everyday life of every human being? Psychiatry's basic discoveries and objectives are clearly explained, and then are shown in their relation to everyday living. Cartoons, 100 of them, and line drawings are effectively used in conveying the concepts of psychiatry that are useful in everyday life.

> . . . we decided to let the cartoons work on the reader and the reader work on himself. When he responds to a cartoon either by a laugh or a frown or a glassy stare of noncomprehension, he will be indicating that something within himself has been stirred by the subject matter of that cartoon. What might this be? Sometimes he will be able to spot it right away; sometimes, not for a while; sometimes, never. But all three kinds of reaction on his part can be significant.

Although limited, by choice of the writers, to only those concepts of psychiatry which are useful in everyday life, *The Inside Story* does deal to a degree with the psychiatrists themselves.

> . . . the ideal psychiatrist would be a warm and intelligent person, with a good marriage of his or her own, blessed, if possible, with children. He or she would have been given thorough technical training at a good medical school, and would have picked up a broad cultural viewpoint. For a doctor to be helpful to patients, he must be able to combine common sense (developed through his own living) with uncommon sense (technical training in diagnosis and treatment of mental illnesses); both are essential.

To anyone (layman, intern, or doctor) interested in a better understanding of human beings and their problems, I recommend this book. Just as whole-heartedly, I recommend it to the reader who would enjoy exploring the facts (known and unknown) that are a part of his own total personality, because as June Bingham wrote, "Everybody, the psychiatrist included, is bound to grow up with a certain amount of emotional astigmatism."

JAMES L. MCCARTNEY

Garden City, New York

Fundamental Principles of Bacteriology. A. J. Salle. McGraw-Hill, New York–London, ed. 4, 1954. ix + 782 pp. Illus. $8.50.

Laboratory Manual on Fundamental Principles of Bacteriology. A. J. Salle. McGraw-Hill, New York–London, ed. 4, 1954. x + 176 pp. Illus. + plates. $3.50.

THIS is the fourth edition of a textbook on beginning bacteriology for students who have had biochemistry or organic chemistry. Obviously it is intended for students who plan to go further in bacteriology, rather than for the majority who take only a beginning course. This edition continues the increase in length that has taken place with each previous edition. The number of figures has been reduced from 297 to 269, permitting the inclusion of more textual material. The chapter on the history of bacteriology has been omitted, but a chapter on the bacteriology of the sea by C. E. ZoBell has been added.

Most of the chapters have been lengthened to include new facts or theories not included in the third edition, and a few chapters have been revised completely. Many of the headings for various sections within chapters have been improved considerably. Structural formulas of organic compounds and chemical equations are complicated and numerous enough to appall the average beginning student who must wonder about the purpose of some of them. So, for example, there are almost eight pages of structural formulas for biological dyes in Table 2 and 24 pages of formulas and equations in Table 21 on the classification of enzymes. Some of the parts on the physiology of bacteria are rather involved for the average beginning student.

The long lists of references at the end of chapters are helpful to the instructor and might prove of some interest to an advanced student, but probably would be used little, if any, by the average beginner, who could make better use of a few general references. Chapter 13, for example, lists some 141 references, and chapter 24 gives 148.

Most chapters more than adequately cover the respective subjects and, in fact, often deal with them in great detail. The weakest chapter is that on the bacteriology of food, where technology crowds out most bacteriology. Examples of errors include the storage of eggs at $-6°C$, the blanching of fruits and the inclusion of chemical preservatives that are not only illegal but positively harmful to health. No clear distinction is made between true poisoning from foods and infections. This chapter and the one on dairy bacteriology are not as well organized or as accurate as most others. The description of bacterial diseases of man is by bacterial genera that have been arranged alphabetically. This is convenient for the author, but is not helpful to the student, who would appreciate a better basis of grouping than an alphabetical one. It is surprising, too, to find viral diseases of man separated from bacterial diseases by two other chapters.

This textbook should prove very useful to teachers

of beginning bacteriology, for it presents much information beyond that usually presented to the beginner. It should serve as a valuable reference book for the ambitious beginner who desires more information on special subjects than that provided by most beginning textbooks as well as a list of original references, and it is especially well adapted to the needs of graduate students who must take a beginning course.

THE author states that *Laboratory Manual on Fundamental Principles of Bacteriology* has been written to accompany the fourth edition of his textbook *Fundamental Principles of Bacteriology* and is intended for use by all groups of students except, perhaps, those in medicine. The 98 experiments are sufficient for a one-semester course of six laboratory-hours per week.

Like the textbook, the manual is written primarily for students who plan to take advanced work in bacteriology; however, selection of experiments would enable use of the manual for a survey course. The experiments cover the subjects well and keep them in balance. Directions for experiments are succinct and the questions are apropos. Very useful to the teacher is the appendix with its descriptions of the preparation of solutions, stains, and culture mediums.

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Introduction to Mathematical Statistics. Paul G. Hoel. Wiley, New York; Chapman & Hall, London, ed. 2, 1954. xi + 331 pp. Illus. $5.

IN the teaching of an introductory course in statistics one must decide upon one of three possible approaches: (i) emphasize the theory, (ii) emphasize the applications, or (iii) strike a balance between theory and practice. This textbook adopts the third approach and does a very good job of it. The author has in his first three chapters an excellent introduction to probability and statistics for the student with a calculus background. The changes from the first edition have all tended to improve the text. The new emphasis placed on regression seems well worth while. The later chapters, including those on small sample distributions, statistical design in experiments, and nonparametric methods cover many of the more modern approaches of statistical methods. One may question if it is possible to do them justice in a one-year course since the 327 pages of the textbook present quite a challege to the average student, but they may serve as future reading for students if one is unable to complete it in the prescribed time. The existence of extensive problem sets for each chapter is another strong point. It is to be expected, however, that the success of the publication as an introductory textbook will depend a great deal on the mathematical maturity of the student. Students who have had trouble with calculus will not find the material easy going.

CARL F. KOSSACK
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General Chemistry. P. W. Selwood. Holt, New York, rev. ed., 1954. xii + 657 pp. Illus. $6.

THE revision of this popular textbook has occurred principally in the new chapter on ionic equilibriums and in the material on ionization in general. Thus such paragraphs as equilibrium constant, *p*H and the ionization of water, normal solutions, equation writing, and single electrode potentials have been added. In general the treatment has been made more quantitative than in the first edition. The exercises at the end of each chapter have been enlarged. These are carefully designed to help the student test his knowledge of the material covered and are one of the many excellent teaching devices for which the book is noteworthy. The material on organic chemistry is especially extensive and should make the textbook of especial appeal to those whose major interests lie in other fields.

JOHN A. TIMM
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Aspects of Culture and Personality. A symposium. Francis L. K. Hsu, Ed. Abelard-Schuman, New York, 1954. xiii + 305 pp. $4.

THIS book begins with nine presentations by three psychologists (Otto Klineberg, T. W. Richards, O. H. Mowrer), three anthropologists (John Gillin, Jules Henry, Ralph Linton), one sociologist (Manford Kuhn), one psychiatrist (Benjamin Boshes), and an additional contributor (G. K. Yacorzynski) who is not identified by profession but who could well be a theologian or philosopher. A discussion is led off by someone from another field and followed by comments from other members of the symposium, which met at Northwestern University in 1951. There follows a chapter evaluating the present state of culture and personality studies from the points of view of social psychology (Robert Winch), anthropology (Adamson Hoebel), psychiatry and psychoanalysis (Clara Thompson), and clinical psychology (William A. Hunt). The final chapter records a general discussion among the 30 or more members of the symposium.

Published symposiums are notably uneven in quality, but this one is exceptionally so. Some of the original presentations are essentially empty of data; others consist of hardly more than raw data. On the other hand, the paper by Henry compels respect because it introduces both relevant data and fresh ideas. Much of the discussion is trite or diffuse or both. Much space is wasted through patent communication difficulties among the participants. Every now and then there is a nice point which one is as happy to find as a plum in a pudding, but for the most part this book makes dreary reading. One must explicitly question whether its publication between hard covers was justified. A duplicated record for the members of the group and for distribution to perhaps 100 persons not present would have sufficed if possibly three papers of substance had been published as journal articles. Even the index is grossly inadequate.

Furthermore, the present volume does not cheer the reader with regard to the scientific prospects of this field of study. Perhaps the best that can be said is voiced (p. 210) by Ralph Linton: "Although we really know very little today, we are in a much better position than we ever have been to find out."

CLYDE KLUCKHOHN

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Science and Civilisation in China. vol. I: *Introductory Orientations.* Joseph Needham. Cambridge Univ. Press, New York, 1954. xxxviii + 318 pp. Illus. + plates + maps. $10.

HISTORIANS of science have known for some time that Joseph Needham, reader in biochemistry at Cambridge University, has been working on a monumental history of Chinese science. At last the good news is announced that he has completed the entire manuscript and that the work is being published seriatim in seven volumes. Volume I, subtitled "Introduction and orientations," is only the overture to a major opus, but it is enough to show that our knowledge of China and Chinese culture will be on a much higher plane when all seven volumes are published.

An analytic table of contents of the whole work shows that it is divided into 50 sections whose length and importance will vary considerably. The first seven sections, published in volume I, are devoted primarily to giving the background and cultural surroundings of Chinese science. The background is, and should be, all inclusive and shows that the word *civilisation* in the title is fully justified. The development of science, of course, should be described in its historical, philosophic, and intellectual setting. Indeed, the chief value of this first volume lies in the fact that it presents the growth of science as part of a cultural complex. The civilization of China is traced from its legendary beginnings to the 17th century, when the contacts of China with Europe brought Chinese science into world science. Volume I thus sets the stage and the stage setting is, of course, an essential part of any historical work.

The volume opens with a description of the scholarly apparatus used by the author and the techniques he used to discover the Chinese contributions to science. This is followed by (i) a geographic and (ii) a historical introduction. The most detailed section (7) is entitled "Conditions of travel of scientific ideas and techniques between China and Europe." The volume ends with an appendix containing three copious bibliographies and a detailed index.

The titles of the subsequent volumes indicate the scope of the work. Volume II is *History of Scientific Thought* (sections 8–18); III is *Mathematics and the Sciences of the Heavens and the Earth* (sections 19–25); IV is *Physics, Engineering and Technology* (sections 26–32); V is *Chemistry and Industrial Chemistry* (sections 33–37); VI is *Biology, Agriculture and Medicine* (sections 38–45); and the final volume is entitled *The Social Background* (sections 46–50). For the first time, a really comprehensive history of science in China will be recorded in a single work.

A review of volume I should be primarily descriptive, because it cannot be evaluated finally until the whole is published. It is enough of a sample, however, to indicate the author's qualifications and his method of presenting his material. Needham obviously is competent. He has lived in China for many years; he is a sinologist, as well as a biochemist and a historian of science. His scholarship is adequate for his complicated task and he evidently loves his subject. I can express only my impatience for the remaining volumes to appear.

CONWAY ZIRKLE

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Atomic Energy: A Survey. J. Rotblat, Ed. Taylor & Francis, London, 1954. (For the Atomic Scientists' Association.) viii + 72 pp. Illus. + plates. Cloth: £6 6s.; paper: £4 6s.

THIS short book is a collection of lectures delivered in early 1954 at the University of London. The lectures were for the general public and as such are not highly technical. They were delivered as a status report on the state of the atomic energy business by leading authorities in the various fields discussed.

Each lecture shows great depth and understanding of the subject by the speaker plus an excellent choice of level at which to pitch a popular discussion. The subjects cover rather well the fields of popular appreciation ranging from a description of research at Harwell through a discussion of atomic weapons and nuclear power to the medical uses and moral issues of atomic energy.

I was particularly interested in the chapter by Simon on "Power from atomic energy." His analyses of the present power situation, the state of the nuclear power act, and the inevitable pattern of the utilization of a new power source are both succinct and timely. Of particular interest are the comparisons among the various countries of their present position and future prospects.

The other noteworthy chapter is by Loutit on "Radiation hazards of atomic energy." He discusses this problem in its proper context, that is, by comparing it with already existing health problems such as the dust hazard caused by coal mines, the smoke hazard caused by factories, and the explosive hazards involved in the oil industry. Loutit's conclusion is that, in the long run, we can adequately cope with the fission products which constitute the main hazard of a nuclear energy power economy in any one of several ways.

The remaining chapters are well written and informative but I found the survey of work at Harwell, the summary of atomic weapons, and the report on the medical uses of atomic energy considerably less stimulating than the subjects previously cited.

Finally, a mention should be made of the two short talks given by Kathleen Lonsdale and George Thom-

son on the moral issues of atomic energy. The speakers ably represent the depth of thought that has been given by nuclear energy scientists to the philosophic aspects of nuclear weapons. As with all philosophic problems, it is impossible for a large group to come to a single definitive conclusion on what course one should follow in such a difficult matter as the moral aspects of the production and/or use of nuclear weapons. Nevertheless, both of these speakers give some very thought-provoking ideas on the subject. This is undoubtedly a very excellent topic with which to close such a series of public discussions.

On the whole, this collection is very well done and I would like to recommend that in the future American scientific groups consider making such a report to the people in this country. Without a doubt, we have seriously failed for a long time to measure up to our British colleagues in this very important area of public relations and education.

ROBERT A. CHARPIE

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Human Development. John P. Zubek and P. A. Solberg. McGraw-Hill, New York–London, 1954. vii + 476 pp. Illus. $6.

IN *Human Development* the authors have produced a textbook that will find wide use. In it they have considered the entire life span from conception to senility and death. Their statement that the book is strongly physiologically oriented is well founded. A chapter on glandular development contains a well organized statement of the anatomy and function of both ductless and secretory glands, of the nature and action of the enzymes, and of the relationship of certain endocrine disorders to behavioral changes. Enzymatic changes, both genetic and resulting from nutritional deficiency, are mentioned along with their effect on behavior. Neural anatomy and physiology are well covered. A brief but well-organized section on genetics presents mechanisms and the simpler laws of inheritance. The genetic foundations of behavior are emphasized.

The book is organized as a parallel description of phylogenetic and ontogenetic changes based upon the authors' belief that no thorough understanding of human development can be acquired without a knowledge of phylogenetic development. This approach, an effective one, coupled with the meticulous organization and the clarity of writing make the book readable and in the main enlightening.

While *Human Development* was written by psychologists, it is strangely enough least effective in the areas of personality and social emotional adjustment. For example, the entire subject of a child's relationship to his parents as a determinant of personality is disposed of in exactly 14 lines. While there are statements on the effect of estrogens and other steroids on senile skin, and these references appear again in the bibliography, the name of Sigmund Freud does not appear. "Unconscious" is not to be found in the subject index. Such omissions sharply limit the value of the book as an instrument for teaching psychology, as does the fact that the authors have attempted to include too many bits of information on personality and social adjustment. The result is that in these sections no framework or concepts emerge, and the student reading them is likely to have no more concept of what constitutes personality and how it is developed than he already had.

LESTER W. SONTAG

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The Geometry of René Descartes. Trans. from the French and Latin by David Eugene Smith and Marcia L. Latham with a facsimile of the first edition 1637. Published by Open Court, Chicago, 1925. Reprinted by Dover, New York, 1954. xiii + 244 pp. Illus. Paper, $1.50; cloth, $2.95.

THIS translation belongs to the series of Dover reprints in the history of science. In view of its great importance for the teaching of analytic geometry, it was reviewed in various American and foreign journals, for example, the *Mathematical Monthly* and *L'Enseignement mathématique*, at the time of its publication in 1925. In the periodical *Isis*, devoted to the history of science, the eminent George Sarton gave an informative review (vol. 8, 1926; pp. 173–175) from which we quote:

> . . . this facsimile edition is splendid and extremely convenient . . . the translators have tried to give the meaning of the original in simple English. . . . the very handsome volume . . . will greatly facilitate the task of anyone having to teach the history of mathematics to English speaking students.

The present Dover reprint will be welcome, since it makes the original edition available at a much lower price.

S. H. GOULD

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The Present State of Physics. A symposium presented on Dec. 30, 1949 at New York meeting of the AAAS. Arranged by Frederick S. Brackett. AAAS, Washington, D.C., 1954. vi + 265 pp. Illus. $6.75 (Members, $5.75.)

THIS book consists of the following four sections, with subdivisions and the pages on which they begin as indicated.

Elementary Particles: "The magnetic moment of the electron" (p. 3), P. Kusch; "Cosmic ray experiments at high altitude" (p. 11), Edward P. Ney; "Developments in cosmic radiation, 1945–1950" (p. 28), J. C. Street.

Physics of the Solid State: "The new electronics" (p. 57), Karl Lark-Horovitz; "Flow of electrons and holes in semiconductors" (p. 128), J. Bardeen; "Barium titanate ferroelectrics" (p. 150), A. von Hippel.

Chemical Physics: "The structure of polymers" (p. 169), Peter J. W. Debye; "Implications of the chemical kinetics of some biological systems" (p. 189), Rufus Lumry and Henry Eyring.

Bio ...s: "Some physical and chemical properties of ax... ...lated to conduction of nerve impulses" (p. 213), ...k Brink, Jr.; "Bioluminescence and the theory of reac... ...rate control in living systems" (p. 238), Frank H. Joh... ...n. Index, p. 263.

The ...ader's attention may first be attracted to (i) choice ... title or subject matter included under the title; (ii) the long period of more than three years between the time of meeting and the appearance of the publication; and (iii) the variation in the nature of treatment of the individual topics.

In considering the first point, there appears to be ample justification for questioning the choice of title. A prospective reader may be misled to anticipate a comprehensive treatment of all fields of physics rather than 10 lectures on selected topics in four groups, two of which are in borderline fields with other sciences.

The long period of more than 3 years between the time of the original lectures and the date of publication is unfortunate and may raise a question regarding use of the word "Present" in the title. Although some of the lecture material was revised subsequent to the time of delivery, this was not done in all cases. Thus, one must answer for himself the question regarding the date of writing.

The nature of the treatment of the individual topics varies markedly as we pass from author to author. While this characteristic may be anticipated to some extent in such a series of lectures, it nevertheless places a limitation on the usefulness and application of the material. One finds, for example, that supplementary references to original literature vary between the extremes of none for the second and seventh lectures to 352 for the fourth lecture! Correspondingly, in some cases the discussion is largely confined to the work of the author, while in others a comprehensive survey of the work in the field is given. In the case of the fourth lecture one finds an interesting and useful historical introduction starting at the very beginning of the subject matter.

The length of the individual discussions can be determined to some extent from the page numbers which have been included in the above table of contents. Many illuminating and useful photographs and diagrams are found throughout the book.

The reader will appreciate the selection of the competent and well-known authorities in the several fields, and the amount and variety of material that has been compressed into a small book of 260 pages. Those who have not followed the developments in borderline fields with other sciences will be impressed with the success and future possibilities in these areas.

All the lectures are recommended for the information and inspiration which may be derived from them. They will give the scientist, whose specialty lies in other fields, brief accounts of the developments in the selected subjects. Some of the lectures, because of their more thorough and general treatment, can be recommended to the serious student, as an introduction to the subject.

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Entomology (Medical & Veterinary). Including insecticides and insect and rat control. D. N. Roy and A. W. A. Brown. Excelsior, Calcutta, ed. 2, 1954. ix + 413 pp. Illus. $8.

THE title, the preface, the eminent Indian and Canadian authors, and the advertising on the cover of this book offer the promise that here is the eagerly awaited treatise on the "one world" approach to the problems of medical and veterinary entomology, so urgently needed in this era of global military operations, world-wide air transport, and World Health Organization. It is indeed regrettable that the work falls so far short of this objective.

The first section of 290 pages is a revision of a previous work on medical entomology by the senior author and to this has been appended a section of 109 pages dealing with insecticides, rodenticides, and their application to the control of specific insect pests. The two parts exist as independent compilations and have not been tied together by unity of style or by cross references. As a result, the book suffers from lack of integration especially since the control of the various insect pests has been discussed in both sections, for example, in two completely independent discussions of malaria control.

The context of the first section is distinctly more medical than veterinary, and relatively superficial coverage is given to such important problems as the mites, lice, ticks, and flies attacking domestic animals and fowl. The emphasis is very largely on oriental problems and one searches in vain for mention of familiar malaria vectors such as *Anopheles quadrimaculatus*, and *A. gambiae*, or of *Culex tarsalis* as a vector of encephalomyelitis.

The coverage of subject matter is very uneven and, despite 30 pages devoted to the biology and taxonomy of Indian anophelines, the subject of malaria transmission is dismissed in less than one-half page and no mention or illustration is made of the four species of plasmodia of importance in human malaria nor are their life-cycles described or figures given. The importance of *Musca domestica* is dealt with in five lines, while its oriental cousins *M. vicina* and *M. nebulo* receive 13 and 11 lines, respectively. Despite the profundity of some chapters, the context of others is inexcusably juvenile and uninformative. The discussion of urticating caterpillars mentions no species, and is illustrated with a figure titled, "A caterpillar with bristly hairs found on mango trees during the rainy season."

Occasional statements, such as "Not all Anopheles are suitable hosts for the plasmodia nor are those species which have become incriminated on either epidemiological or experimental grounds of importance in the transmission of malaria," leave the reader somewhat perplexed.

In contrast to these weak points, the section on the Anopheline mosquitoes of India is detailed and equipped with an excellent series of profusely illustrated keys to adults and larvae. The chapters on

cyclorrhaphous Diptera and mites and ticks are well organized and illustrated and adequate in content.

I have nothing but praise for the second section of the book, which deals with insecticides and insect and rodent control and demonstrates what the entire book might have been with better planning and liaison between the coauthors. This section is well written and arranged and includes much useful information on the properties and uses of all the insecticidal chemicals and their adjuvants. Additional topics which are discussed include the mode of action of insecticides, toxicological hazards of use, methods of evaluation, and apparatus for application. The final two chapters deal effectively with the control of noxious species by chemicals. The second section of the book presents, for the first time in a book of medical and veterinary entomology, a complete and up-to-date discussion of insect control and should be a valuable guide and reference manual for the entomologist, medical man, and veterinarian.

The text of the entire book is pleasingly arranged, well illustrated, and adequately documented as to original source material. Its usefulness is somewhat impaired by inadequate indexing.

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Department of Entomology,
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Astrophysics: Nuclear Transformations, Stellar Interiors, and Nebulae. Lawrence H. Aller. Ronald, New York, 1954. x + 291 pp. Illus. + plates. $12.

WITH the publication of the second volume, L. H. Aller's textbook of astrophysics is now complete. Students have every reason to be grateful that an astronomer of wide interests and experience has taken the time and effort to prepare a modern account of the methods and results of astrophysical research. In fact, it is so up to date that some of the work reported has not yet appeared in the journals.

Unlike the first volume, *Astrophysics—The Atmospheres of the Sun and Stars,* which was largely devoted to a single topic, the present volume explores a wide variety of astrophysical problems. The physical principles derived in the first half of the first volume are here applied to stellar interiors, pulsating stars, shell stars, planetary nebulae, and the interstellar medium. So great is the dependence of the second volume on the first that it is almost impossible to follow a derivation in one without consulting the other. This has the advantage of unifying the subject and permitting many illuminating parallels to be drawn. However, it is also a source of some confusion. Instead of numbering the chapters consecutively through both volumes, the second volume begins anew with chapter 1. Therefore, references back to the first volume require a star and frequently an asterisk and footnote; and in the first volume itself there are references to a nonexistent chapter 10!

With the possible exception of stellar interiors, the subjects dealt with in this volume are not as yet well understood theoretically. For example, the chapter on variable stars presents a formidable array of spectroscopic and photometric observations which a mature theory must eventually explain.

An entire chapter is devoted to the planetary nebulae, reflecting Aller's own interest in these objects. Once again one is impressed with the variety of details that remain to be interpreted.

Unfortunately, many interesting discussions are curtailed by space limitations. However, at the end of each chapter the fundamental references are listed according to topics and the interested student can pursue the argument in the literature.

In keeping with the practice of the first volume, several problems are worked out in detail. For example, the equations of stellar structure are integrated numerically to produce a particular stellar model. In addition, at the end of almost every chapter, a few problems are given. The following is typical: "If the number of Ca II atoms in the line of sight is 3.12×10^{14} per cm^2 while the number of hydrogen atoms in the third level per cm^2 in the line of sight is 5, compare the relative emissions in the Ca II $\lambda 3933$ ($^2S_{1/2} - {}^2P_{3/2}$) line and Hα." The necessary additional information is then supplied.

The textbook would be considerably easier to read if punctuation marks were used at the end of equations in future editions.

Undoubtedly these volumes will be among the first acquisitions of beginning graduate students who will be the principal beneficiaries.

MARSHAL H. WRUBEL

Astronomy Department, Indiana University

Elementary Statistics. Benton J. Underwood, Carl P. Duncan, Janet A. Taylor, and John W. Cotton. Appleton-Century-Crofts, New York, 1954. ix + 239 pp. Illus. $3.25.

IN view of the many elementary statistics books currently available, it should take considerable temerity to bring out a new one. This one, however, has much to recommend it as a textbook in the initial undergraduate statistics course in psychology. In fact, its title would be more informative if it specified (as the preface indicates) that it was primarily for this subject matter field. It is particularly appealing here, for many of the examples used are drawn from published psychological research, and references to sources are cited. It definitely does not appear to be appropriate for the first course in mathematical statistics.

The topics covered are the traditional ones: graphic methods, point and spread measures, the normal curve, sampling, correlation, simple analysis of variance, and Chi square. Nonparametric statistics and test theory are not taken up. The general approach is to develop each concept by means of a common-sense explanation, then present the computational procedures. Commendably, the assumptions and limitations of the statistics are made explicit. This approach also leaves to the in-

structure latitude with respect to the amount of mathematics to present.

The treatment of correlation is particularly good; the explanation of analysis of variance is weakest, although the computational procedures are well presented.

The examples and illustrations are sufficiently numerous and well chosen. An accompanying workbook provides material for practice exercises.

JOHN E. MILHOLLAND
Psychology Department, University of Michigan

Medicine and Science. Lectures to the laity, No. XVI. Iago Galdston, Ed. International Universities Press, New York, 1954. 159 pp. Illus. $3.

EDITOR Iago Galdston has compiled a very pleasant, compact, and readable little volume based on the New York Academy of Medicine lectures to the laity. These authoritative lectures, dealing with current trends in medicine, are directed toward a nontechnical audience. They are very informative and may well engage the attention of all who are generally interested in recent medical developments. Throughout most of the volume, despite the diversity of topics, there runs a thread of continuity that makes for over-all acceptability. The book consists of six lectures with an introduction by H. R. Craig: "Men, machines, and the world about," Norbert Wiener; "The renaissance in endocrinology," Hans Selye (Paul Rosch); "The relation of animal psychology to psychiatry," David M. Levy; "Quest for antibiotics," Paul R. Burkholder; "Stress, emotions and bodily disease," Harold G. Wolff; and "Miracles—mass produced," John E. McKeen.

Wiener, in a light, airy manner, describes the relationship of man to machines, the problems common to both in their mode of action, the mechanism of negative feedback, and the development of the computing machine. The sociologic implications of the machine are also examined.

Selye outlines the general adaptation syndrome concept and how failure of the adaptive mechanism to respond properly to stress may cause disease. The role of the hormones involved in this response mechanism is analyzed.

Levy presents an interesting array of data to support the view that psychological, as well as biological, ties link the human race to the animal species. Absorbing animal psychological studies are cited and the cumulative evidence lends weight to his thesis.

Burkholder traces the story of antibiotics. The lecture covers the nature, source, and history of antibiotics. This is followed by a description of the bacteriological techniques involved, their use in chemotherapy, and the chemical constitution of some of the antibiotics. The problem of drug-fast organisms is also discussed.

Wolff shows how the human body responds to mental stress by evoking an adaptive-protective mechanism that may actually be inimical to the well-being of the individual. Numerous clinical studies are presented involving such responses as diarrhea, constipation, peptic ulcers, headaches, and blood clotting. These body responses are often found to be "far more damaging to the individual than the effects of the noxious agent per se."

McKeen outlines the steps in mass-producing an antibiotic as it would be carried out by a pharmaceutical company. The process starts with a soil sample that contains the fungus spores and ends in the large-scale production of the commercially available antibiotic. The intermediate stages are interestingly pictured.

MORRIS FISHBEIN
Division of Microbiology,
U.S. Food and Drug Administration

Books Reviewed in SCIENCE

7 January

America's Resources of Specialized Talent, Report of the Commission on Human Resources and Advanced Training (Harper). Reviewed by L. A. DuBridge.

Fundamentals of Psychoanalytic Technique, Trygve Brattoy (Wiley; Chapman & Hall). Reviewed by D. Noble.

The Technical Report, B. H. Weil, Ed. (Reinhold). Reviewed by R. Tumbleson.

The Mechanism of Economic Systems. Arnold Tustin (Harvard Univ. Press). Reviewed by J. A. Sawyer.

Radiation Biology, vol. I, pts. 1 and 2, Alexander Hollaender, Ed. (McGraw-Hill). Reviewed by R. H. Chamberlain.

The Horse-Flies (Diptera: Tabanidae) of the Ethiopian Region, vol. II. H. Oldroyd (British Museum of Natural History). Reviewed by C. B. Philip.

Ferromagnetic Domains, K. H. Stewart (Cambridge Univ. Press). Reviewed by J. E. Goldman.

Acoustics, Leo L. Beranek (McGraw-Hill). Reviewed by V. O. Knudsen.

The Kinetic Basis of Molecular Biology, F. H. Johnson, H. Eyring, and M. J. Polissar (Wiley; Chapman & Hall). Reviewed by R. Lumry.

Chemical Constitution, J. A. A. Ketelaar (Elsevier). Reviewed by A. E. Martell.

The Kidney, A. A. G. Lewis and G. E. W. Wolstenholme, Eds. (Little, Brown). Reviewed by A. V. Wolf.

The Moon Puzzle, N. O. Bergquist (Grafisk Forlag). Reviewed by D. ter Haar.

An International Bibliography on Atomic Energy, vol. 2, suppl. 2, United Nations (U.S. Distr.: Columbia Univ. Press).

Gmelins Handbuch der Anorganischen Chemie: Selen, System No. 10, pt. A, sec 3; *Gmelins Handbuch der Anorganischen Chemie: Bor,* System No. 13, supp. vol.; *Gmelins Handbuch der Anorganischen Chemie: Gold,* System No. 62, pts. 2 and 3. Edited by Gmelin Inst. (Verlag). Reviewed by R. Gilchrist.

Proceedings of the Second National Cancer Conference, 1952, vols. I and II. (American Cancer Society). Reviewed by S. Peller.

List of Land Mammals of New Guinea, Celebes, and Adjacent Islands, 1758–1952, Eleanor M. O. Laurie and J. E. Hill (British Museum of Natural History). Reviewed by H. M. Van Deusen.

New Research Techniques in Physics (Academia Brasileira de Ciencias). Reviewed by W. B. Fretter.

14 January

Information Theory in Biology, Henry Quastler, Ed. (Univ. of Illinois Press). Reviewed by P. Cranefield.

Nobel Prize Winners in Medicine and Physiology: 1901–1950. Lloyd G. Stevenson (Schuman). Reviewed by W. R. Amberson.

Rutherford, by Those Who Knew Him (Physical Society). Reviewed by C. Susskind.

Modern Chemical Processes, vol. III, Editors of Industrial and Engineering Chemistry (Reinhold). Reviewed by R. L. Huntington.

Biochemistry, Abraham Cantarow and Bernard Schepartz (Saunders). Reviewed by S. Simmonds.

Metabolic Interrelations, Edward C. Reifenstein, Jr., Ed. (Josiah Macy, Jr. Foundation). Reviewed by D. M. Hegsted.

Introduction to Atomic Physics, Otto Oldenberg (McGraw-Hill). Reviewed by D. C. Peaslee.

21 January

Atomic and Free Radical Reactions, vols. I and II, E. W. R. Steacie (Reinhold). Reviewed by E. H. Taylor.

The Development of Medical Bibliography, Estelle Brodman (Medical Library Assoc.). Reviewed by W. D. Postell.

Physics and Applications of Secondary Electron Emission, H. Bruining (McGraw-Hill). Reviewed by S. Githens, Jr.

The Infrared Spectra of Complex Molecules, L. J. Bellamy (Methuen; Wiley). Reviewed by A. H. Nielsen.

Weather Modification: Past, Present and Future, Kenneth M. Arenberg *et al.* (Weather Modification Group). Reviewed by R. R. Braham, Jr.

Synthetic Methods of Organic Chemistry, vol. 8, W. Theilheimer (Karger; Interscience). Reviewed by C. A. Buehler.

Theoretical Physics, F. Woodbridge Constant (Addison-Wesley). Reviewed by G. G. Harvey.

Biological Effects of External Radiation, Henry A. Blair, Ed. (McGraw-Hill). Reviewed by I. H. Herskowitz.

28 January

Aetiologie und Prophylaxe des Lungenkrebses als ein Problem der Gewerbehygiene und des Tabakrauches (Steinkopff). Reviewed by F. Homburger.

Information Theory, Stanford Goldman (Prentice-Hall). Reviewed by J. Rothstein.

The Energetics of Development, Lester G. Barth and Lucena J. Barth, (Columbia Univ. Press). Reviewed by N. T. Spratt, Jr.

New Fibres from Proteins, Robert L. Wormell (Academic Press; Butterworths). Reviewed by A. M. Sookne.

Sex in Microorganisms, D. H. Wenrich, Ivey F. Lewis, and John R. Raper, Eds. (AAAS). Reviewed by S. Emerson.

The Human Masticatory Apparatus, Meyer Klatsky and Robert L. Fisher (Dental Items of Interest; Kimpton). Reviewed by W. M. Krogman.

Structure Reports for 1945–46, vol. 10, A. J. C. Wilson, Gen. Ed. (Oosthoek). Reviewed by F. J. Llewellyn.

New Books

Introduction to the Theory of Neutron Diffusion. vol. 1. K. M. Case, F. de Hoffmann, G. Placzek. Los Alamos Scientific Lab., Los Alamos, N.M., 1953 (Order from Supt. of Documents, GPO, Washington 25). 174 pp. Paper, $1.25.

Tropical Soils. A critical study of soil genesis as related to climate, rock and vegetation. E. C. J. Mohr and F. A. Van Baren. N. V. Uitgeverij W. Van Hoeve, The Hague–Bandung; Interscience, London–New York, 1954. 498 pp. $8.

Solvents Manual. With solubility chart. C. Marsden, Ed. Cleaver-Hume, London; Elsevier, Houston–New York, 1954. 429 pp. $12.95.

Release of Latent Business. A depression-proof economy; the abolition of unemployment. Emil E. Sundquist. Business Release, Rockwood, Mich., 1954. 160 pp. $2.50.

Organic Syntheses. An annual publication of satisfactory methods for the preparation of organic chemicals. vol. 34. William S. Johnson, Ed. Wiley, New York; Chapman & Hall, London, 1954. 121 pp. $3.50.

Nuclear Species. H. E. Huntley. Macmillan, London; St. Martin's, New York, 1954. 193 pp. $4.50.

The Use of Stereographic Projection in Structural Geology. F. C. Phillips. Edward Arnold, London, 1954 (Distributed by St. Martin's, New York 17). 86 pp. $3.

Modern Science and God. P. J. McLaughlin. Philosophical Library, New York, ed. 2, 1954. 89 pp. $2.75.

Proceedings of the First World Conference on Medical Education, London, 1953. Held under the auspices of the World Medical Association. Hugh Clegg, Ed. Oxford Univ. Press, London–New York, 1954. 804 pp. $16.

The Neuroanatomical Basis for Clinical Neurology. Talmage L. Peele. McGraw-Hill, New York–London, 1954. 564 pp. $12.50.

The Psychological Variables in Human Cancer. Symposium presented at VA Hospital, Long Beach, Calif., 23 Oct. 1953. Joseph A. Gengerelli and Frank J. Kirkner, Eds. Univ. of California Press, Berkeley–Los Angeles, 1954. 135 pp. $3.

Human Limbs and Their Substitutes. Prepared under the sponsorship of the Advisory Committee on Artificial Limbs, Natl. Acad. of Sciences–Natl. Research Council. In summary and correlation of a research program for Dept. of Medicine and Surgery, VA, and for Surgeon General's Office, Dept. of the Army. Paul E. Klopsteg and Philip D. Wilson *et al.* 844 pp. $12.

Soil. G. V. Jacks. Philosophical Library, New York, 1954. 221 pp. $5.

The Ocean Floor. Hans Pettersson. Yale Univ. Press, New Haven; Oxford Univ. Press, London, 1954. 181 pp. $3.

Christianity and Anti-Semitism. Nicolas Berdyaev, Trans. by Alan A. Spears and Victor B. Kanter. Philosophical Library, New York, 1954. 58 pp. $2.75.

Rocket Exploration of the Upper Atmosphere. Special supplement (Vol. 1) to *Journal of Atmospheric and Terrestrial Physics.* R. L. F. Boyd and M. J. Seaton, Eds. Interscience, New York; Pergamon Press, London, 1954. 376 pp. $11.

Biology of Deserts. Proceedings of a symposium. J. L. Cloudsley-Thompson, Ed. Inst. of Biology, London W.C.1, 1954. 224 pp. 14s.

(*Continued on page viii*)

The Mammalian Fetus

Physiological Aspects of Development

Cold Spring Harbor Symposia on Quantitative Biology

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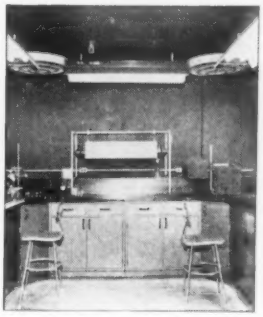

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Recent Wiley Publications

SONICS: Techniques for Industrial Uses of Sound and Ultrasound

By Theodor F. Hueter and Richard H. Bolt, both of The Massachusetts Institute of Technology. A unified presentation of the analysis, testing, and processing of materials and products by the use of acoustical energy. The book covers techniques and devices ranging from a few cycles per second to hundreds of megacycles per second. Its material is drawn from physics, engineering, and electronics and its emphasis is on the practical applications. Typical examples are selected to illustrate operating principles and suggest further uses. *1955. 456 pages. 231 illus. $10.00.*

THEORIES OF PERCEPTION AND THE CONCEPT OF STRUCTURE

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By Floyd H. Allport, Syracuse University. This unique, dual purpose volume provides the first comprehensive, organized survey of the major current theories of perception, and offers a foundation for systematic work in the form of a general theory of structure in behavior. *1955. 709 pages. $8.00.*

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(Continued from page 210)

British Pharmaceutical Codex, 1954. Pharmaceutical Press, London, 1954. 1340 pp £3 3s.

Perspectives in Physiology. An international symposium, 1953. Elza Veith, Ed. American Physiological Society, Washington 14, 1954. 171 pp. $3.

Principles of Biochemistry. Abraham White, Philip Handler, Emil L. Smith, and DeWitt Stetten, Jr. McGraw-Hill, New York–London, 1954. 1117 pp. $15.

From the World of the Cabbalah. The philosophy of Rabbi Judah Loew of Prague. Ben Zion Bokser. Philosophical Library, New York, 1954. 210 pp. $3.

Fossil Amphibians and Reptiles. W. E. Swinton, British Museum (Natural History), London, 1954. 144 pp. Paper, 5s.

Birds the World Over. As shown in habitat groups in Chicago Natural History Museum. Austin L. Rand and Emmet R. Blake. Chicago Natural History Museum, Chicago, 1954. 96 pp. Paper, $1.50.

Advances in Food Research. vol. V. E. M. Mrak and G. F. Stewart, Eds. Academic Press, New York, 1954. 538 pp. $11.50.

180 Games for One Player. Compiled by J. B. Pick. Philosophical Library, New York, 1954. 137 pp. $3.75.

Tables of Integral Transforms. vol. II. Based in part on notes left by Harry Bateman. Bateman Project staff, A. Erdélyi, Ed. McGraw-Hill, New York–London, 1954. 451 pp. $8.

Vorlesungen Über Differential- und Integralrechnung. Mathematische Reihe, Band VII. A. Ostrowski. Integralrechnung auf dem Gebiete mehrerer Variablen, Band III. Birkhauser, Basel-Stuttgart, 1954. 475 pp. Paper, F. 73.85; cloth, F. 78.

Sailing Aerodynamics. John Morwood. Philosophical Library, New York, 1954. 124 pp. $7.50.

Advances in Catalysis and Related Subjects. vol. VI. W. G. Frankenburg, V. I. Komarewsky, and E. K. Rideal, Eds. Academic Press, New York, 1954. 467 pp. $10.50.

Treatise on Invertebrate Paleontology. Raymond C. Moore, Ed. pt. D, *Protista 3: Protozoa (Chiefly Radiolaria and Tintinnina)*. Arthur Shackleton Campbell and Raymond C. Moore. Geological Society of America; Univ. of Kansas Press, Lawrence, 1954. 195 pp. $3.

Blood Groups in Man. R. R. Race and Ruth Sanger. Thomas, Springfield, Ill., ed. 2, 1954. 400 pp. $7.50.

Laboratory Techniques in Rabies. Monogr. Ser., No. 23. World Health Organization, Geneva, 1954 (Distributed by Columbia Univ. Press, New York 27). 150 pp. Paper, $3; cloth, $4.

Cellulose and Cellulose Derivatives. pt. II. Emil Ott, Harold M. Spurlin, and Mildred W. Grafflin, Eds. vol. V of *High Polymers*. Interscience, New York–London, rev. ed. 2, 1954. 545 pp. $12.

Biology of Anopheles gambiae. Research in French West Africa. Monogr. Ser., No. 9. M. H. Holstein. World Health Organization, Geneva, 1954 (Distributed by Columbia Univ. Press, New York 27). 172 pp. Paper, $2.

American Game Birds of Field and Forest. Their habits, ecology and management. Frank C. Edminster. Scribner's New York, 1954. 490 pp. $12.50.

The Vitamins: Chemistry, Physiology, Pathology. vol. III. W. H. Sebrell, Jr., and Robert S. Harris, Eds. Academic Press, New York, 1954. 665 pp. $15.

Treasury of Philosophy. Dagobert D. Runes, Ed. Philosophical Library, New York, 1954. 1280 pp. $15.

Meetings

March

20–23. American Inst. of Chemical Engineers, Louisville, Ky. (S. L. Tyler, AICE, 120 E. 41 St., New York 17.)

23–26. International Academy of Proctology, 7th annual, New York. (IAP, 43–55 Kissena Blvd., Flushing, N.Y.)

24–26. National Science Teachers Assoc., Cincinnati, Ohio. (R. H. Carleton, 1201 16 St., NW, Washington, D.C.)

25–26. Michigan Acad. of Science, Arts and Letters, E. Lansing. (George M. McEwen, Univ. of Michigan, Ann Arbor.)

28–31. American Assoc. of Petroleum Geologists, New York, N.Y. (E. H. Powers, Box 670, Fort Worth, Tex.)

28–1. Western Metal Exposition, 9th, Los Angeles, Calif. (W. H. Eisenman, 7301 Euclid Ave., Cleveland 3, Ohio.)

29–7. American Chemical Soc., 127th national, Cincinnati, Ohio (A. H. Emery, 1155 16 St., NW, Washington 6.)

31–2. Soc. of Research in Child Development, Monticello, Ill. (C. B. Stendler, College of Education, University of Illinois, Urbana.)

April

1–5. Japan Medical Cong., Kyoto. (M. Goto, University Hospital, Kyoto Univ., Kyoto.)

2. Kappa Delta Pi, Cleveland, Ohio. (E. I. F. Williams, 238 E. Perry St., Tiffin, Ohio.)

3–6. American Astronomical Soc., Princeton, N.J. (C. M. Huffer, Washburn Observatory, Madison 6, Wis.)

3–7. American College Personnel Assoc., Chicago, Ill. (C. Evans, Univ. of Indiana, Bloomington.)

4. American Educational Research Assoc., Cleveland, Ohio. (F. W. Hubbard, 1201 16 St., NW, Washington 6, D.C.)

4–5. National Gastrointestinal Cancer Conf., 6th New York, N.Y. (Morris K. Barrett, National Institutes of Health, Bethesda 14, Md.)

4–6. American Assoc. of Physical Anthropologists, Philadelphia, Pa. (J. L. Angel, Jefferson Medical College, 307 S. 11 St., Philadelphia 7.)

4–6. International Meeting on Phlebology, 3rd, Aix-en-Provence, France. (F. Beurier, 4 Cours Mirabeau, Aix-en-Provence.)

5. Histochemical Soc. Symposium, Philadelphia, Pa. (A. B. Novikoff. Waldemar Medical Research Foundation, Port Washington, N.Y.)

5. Microcirculatory Conf. on Physiology and Pathology, 2nd, sponsored by American Assoc. of Anatomists, Philadelphia, Pa. (G. P. Fulton, College of Liberal Arts, Boston Univ., Boston 15.)

5. Tissue Culture Assoc., annual, Philadelphia, Pa. (M. R. Murray, TCA, College of Physicians and Surgeons, New York 32.)

5–7. French Medical Cong. on Thrombosis and New Antibiotics, 30th Algiers, Algeria. (R. Raynaud, 12 Rue A.-Leluch, Algiers.)

5–7. Radio Technical Commission for Aeronautics, spring assembly meeting, Los Angeles, Calif. (RTCA, Room 2036, Building T-5, 16th and Constitution Ave., NW, Washington 25.)

6–8. American Assoc. of Anatomists, Philadelphia, Pa. (N. L. Hoerr, 2109 Adelbert Rd., Cleveland 6, Ohio.)

8–9. Pen[illegible]vania Academy of Science, Philadelphia, Pa. (K. D[illegible]lf, Public Museum and Art Gallery, Reading, Pa.)

9. South[illegible]arolina Acad. of Science, Columbia. (H. W. Freem[illegible]. Univ. of South Carolina, Columbia.)

10–15. A[illegible]erican Inst. of Homeopathy, Washington, D.C. (W. R[illegible] Huntsman, AIH, 1601 Chestnut St., Philadelphia 3, Pa.)

10–15. American Inst. of Nutrition, San Francisco, Calif. (R. W. Engel, Dept. of Biochemistry and Nutrition, Virginia Polytechnical Inst., Blacksburg.)

10–16. American Physiological Soc., San Francisco, Calif. (M. O. Lee, 2101 Constitution Ave., Washington 25, D.C.)

10–16. American Soc. for Experimental Pathology, San Francisco, Calif. (C. C. Erickson, Inst. of Pathology, Univ. of Tennessee, 858 Madison Ave., Memphis.)

10–16. Federation of American Societies for Experimental Biology, San Francisco, Calif. (M. O. Lee, 2101 Constitution Ave., Washington 25, D.C.)

10–18. International Urological Soc., 10th cong., Athens, Greece. (Z. Kaires, Rue Voukourestiou 25, Athens.)

11–14. Assoc. of American Geographers, annual, Memphis, Tenn. (B. W. Adkinson, Library of Congress, Washington 25, D.C.)

11–14. International Poplar Commission, 8th session, Madrid, Spain. (Secretary, c/o Forestry Div., FAO, Villa Borghese, Rome, Italy.)

11–15. American Assoc. of Immunologists, annual, San Francisco, Calif. (F. S. Cheever, Graduate School of Public Health, Univ. of Pittsburgh, Pittsburgh 13, Pa.)

11–15. American Soc. of Biological Chemists, San Francisco, Calif. (P. Handler, Duke Univ., School of Medicine, Durham, N.C.)

12–15. International Union of Biological Sciences, 12th general assembly, Rome, Italy. (P. Weiss, 2101 Constitution Ave., Washington 25, D.C.)

13–15. Conf. on Biological Waste Treatment, New York, N.Y. (W. W. Eckenfelder, Civil Engineering Dept., Manhattan College, New York 71.)

13–16. Mathematical Assoc., Leicester, England. (F. W. Kellaway, 87 Pixmore Way, Letchworth, Herts.)

14. World Meteorological Organization, 2nd cong., Geneva, Switzerland. (G. Swoboda, WMO, 1, Ave. de la Paix, Geneva.)

14–15. High Polymer Forum, 6th Canadian, Ontario, Canada. (D. G. Ivey, Dept. of Physics, Univ. of Toronto, Toronto.)

14–16. National Speleological Soc., Natural Bridge, Va. (E. Moffett, 3047 S. Columbus St., Arlington, Va.)

15–16. American Mathematical Soc., Brooklyn, N.Y. (AMS, 80 Waterman St., Providence 6, R.I.)

15–16. Eastern Psychological Assoc., Philadelphia, Pa. (G. Lane, Dept. of Psychology, Univ. of Delaware, Newark.)

15–16. Iowa Acad. of Science, Davenport. (J. L. Laffoon, Iowa State College, Ames.)

15–16. Louisiana Acad. of Science, New Orleans. (E. S. Hathaway, 1423 Jefferson Ave., New Orleans 15.)

15–16. Montana Acad. of Science, Butte. (L. H. Harvey, Dept. of Botany, Montana State Univ., Missoula.)

15–17. Italian Soc. of Geophysics and Meteorology, 3rd general assembly, Genoa. (I. Dagnino, 5 Via Balbi, Genoa.)

16. Committee for the Scientific Study of Religion, spring, New York, N.Y. (Walter H. Clark, Hartford Seminary Foundation, Hartford 5, Conn.)

17–20. American Oil Chemists' Soc., 46th annual, New Orleans, La. (C. L. Hoffpauir, 2100 Robert E. Lee Blvd., New Orleans 19.)

18–19. National Air Pollution Symposium, 3rd, Pasadena, Calif. (A. M. Zarem, 621 S. Hope St., Los Angeles 17, Calif.)

21–22. Eastern States Health Education Conf., New York Acad. of Medicine, New York, (NYAM, 2 E. 103 St., New York 29.)

21–23. Assoc. of Southeastern Biologists, annual, Charleston, S.C. (M. E. Gaulden, Biology Div., Oak Ridge National Laboratory, Oak Ridge, Tenn.)

21–23. Ohio Acad. of Science, annual, Ohio Wesleyan Univ., Delaware. (R. W. Dexter, Kent State Univ., Kent, Ohio.)

22. American Eugenics Soc., annual, New York, N.Y. (AES, 230 Park Ave., New York 17.)

22–23. Arkansas Acad. of Science, annual, Searcy. (L. F. Bailey, Univ. of Arkansas, Fayetteville.)

22–23. American Mathematical Soc., Chicago, Ill. (E. G. Begle, Yale Univ., New Haven 11, Conn.)

22–23. Georgia Acad. of Science, Athens. (W. B. Redmond, Box 534, Emory University, Ga.)

24–26. American Association for the Advancement of Science, southwestern division, Sante Fe, N.M. (F. E. E. Germann, Dept. of Chemistry, Univ. of Colorado, Boulder.)

24–29. Inter-American Cong. of Radiology, 5th Washington, D.C. (E. P. Pendergrass, 3400 Spruce St., Philadelphia 4, Pa.)

25–27. National Acad. of Sciences, 92nd annual, Washington, D.C. (Secretary, NAC, 2101 Constitution Ave., NW, Washington 25.)

25–30. International Symposium on Electrical Discharges in Gases, Delft, Netherlands. (A. W. van Wagensveld, Mijnbouwplein 11, Delft.)

26–28. American Industrial Hygiene Assoc., Buffalo, N.Y. (H. F. Smyth, Jr., 4400 5 Ave., Pittsburgh 13, Pa.)

26–29. International Health Cong., Bournemouth, England. (P. A. Wells, 90 Buckingham Palace Rd., London, S.W.1.)

26–1. International Symposium on Arid Land Problems, Albuquerque, N.M. (J. A. Behnke, 1515 Massachusetts Ave., NW, Washington 5, D.C.)

27–29. International Conf. on Nuclear Engineering, Los Angeles, Calif. (T. J. Connolly, Dept. of Engineering, Univ. of California, Los Angeles 24.)

27–29. Soc. for Experimental Stress Analysis, Los Angeles, Calif. (W. M. Murray, Central Square Station, P.O. Box 168, Cambridge 39, Mass.)

28–30. American Physical Soc., Washington, D.C. (K. K. Darrow, Columbia Univ., New York 27.)

28–30. Midwestern Psychological Assoc., Chicago, Ill. (L. J. Cronbach, 1007 S. Wright, Champaign, Ill.)

29–30. Alabama Acad. of Science, Tuscaloosa. (H. McCullough, Howard College, Birmingham, Ala.)

29–30. Colorado-Wyoming Acad. of Science, Greeley, Colo. (O W. Olsen, Colorado A. & M. College, Fort Collins.)

29–30. Mississippi Acad. of Sciences, Meridian. (C. Q. Sheely, Box 574, State College, Miss.)

29–30. Seismological Soc. of America, Berkeley, Calif. (G. D. Louderback, Bacon Hall, Univ. of California, Berkeley 4.)

30. American Mathematical Soc., Stanford Univ., Calif. (E. G. Begle, Yale Univ., New Haven 11, Conn.)

INTERNATIONAL ARID LANDS MEETINGS

of the AAAS and its

SOUTHWESTERN AND ROCKY MOUNTAIN DIVISION

University of New Mexico, Albuquerque, New Mexico, April 26-29

Southwestern Field Trip, Albuquerque-El Paso and return, April 30-May 1

Featuring a **Symposium on What is the Future of Arid Lands?** Public evening lectures, April 26, 27 and 28 by Homer L. Shantz, Charles E. Kellogg, and B. T. Dickson (Australia), and Technical Sessions:

Morning, April 27—Variability and Predictability of Water Supply in Arid Regions—Speakers: Reed W. Bailey, C. W. Thornthwaite, Jean Tixeront (Tunisia), Luna Leopold, F. Dixey (England), C. C. Wallén (Sweden), John H. Dorroh, T. L. Smiley.

Afternoon, April 27—Better Use of Present Resources—Speakers: Kanwar Sain (India), R. O. Whyte (Italy), L. N. McClellan, Hilgard O'R. Sternberg (Brazil), L. A. Richards, Cyril Luker, Raymond Price, Pedro Armillas (Mexico).

Morning, April 28—Prospects for Additional Water Sources—Speakers: E. J. Workman, Edward Bowen (Australia), Sheppard T. Powell, W. F. J. M. Krul (Holland), Vincent J. Shaefer, Glenn W. Brier, H. E. Hayward, Louis Koenig.

Afternoon, April 28—Better Adaptation of Plants and Animals to Arid Conditions—Speakers: Olaf S. Aamodt, Colonel Omar Draz (Egypt), R. Merton Love, Knut Schmidt-Nielsen, Enrique Beltrán (Mexico), B. P. Uvarov (England), Michael Evenari (Israel), L. M. Pultz.

Discussion Group Sessions, April 29 (See SCIENCE of February 11, 1955, or THE SCIENTIFIC MONTHLY of March, 1955, or write to the American Association for the Advancement of Science, 1515 Massachusetts Avenue, N.W., Washington 5, D. C., U.S.A., for details. Cable Address: Advancesci, Washington, D. C.)

Housing Reservation

International Arid Lands Meetings Housing Bureau
Chamber of Commerce, Albuquerque, New Mexico

Please send requests *directly* to the Bureau to save time. Confirmation in two weeks in order received. *Single rooms may become scarce; if possible share a twin-bedded room—and also save.* Be sure to indicate if you need a room the night of May 1 on return from the Field Trip. *Deadline for receipt of reservations,* **April 15.**

Hotels	*Single with bath*	*Double with bath*	*Twin-bedded with bath*
HILTON	$7.50	$9.00	$10.00
FRANCISCAN	5.50	6.60	9.50
ALVARADO	5.50–9.00 (only a few 5.50)	.....	8.50–12.00
EL FIDEL	4.50 (a few 3.50 & 4.00 without bath)	5.00–6.00 (a few 3.50 & 4.50 without bath)	8.00 (a few 5.00 & 7.00 without bath)
Motels (all with bath)	$4.00–$5.00	$5.00–$6.00	$7.00–$10.00

All of the above prices subject to 2% state tax.

Please reserve the following accommodations: Date of Application: .....
State name of hotel or insert "Motel" if this is your preference:
First Choice ..... Second Choice ..... Third Choice .....
.....Single room with bath Desired rate ..... Maximum rate .....
.....Double-bedded room with bath Desired rate ..... Maximum rate .....
.....Twin-bedded room with bath Desired rate ..... Maximum rate .....
The name and address of each person, including yourself, must be listed (attach list if necessary.) .....
.....
Date and time of arrival ..... Date and time of departure .....
Mode of Transportation .....
Name .....
(Please print or type name of individual making this request.)
Address .....
(Street) (City and Zone) (State)
Enclose deposit of $10.00 (refunded if cancelled before April 15.)
Make checks payable to Berl Huffman, Secretary, Albuquerque Chamber of Commerce.

Registration Form

International Arid Lands Meetings
(All Registration by Mail in Advance. No fee.)

Name (Dr., Miss, etc.) .....
(Please print or type) (Last) (First) (Initial)
Academic, Professional or Business Connection and Title .....
Office or Home Address (Mail Address) .....
.....
Field of Interest .....
Probable Discussion Group Preference (by number) .....
(You will be free to change or to attend more than one.)
Field Trip Reservation (limited to first 222 applicants—deadline **April 5**)
Yes ..... No ..... ($20.00 for trip, lodging, and meals *must* accompany reservation. Refunds up to April 15.)
Send to: Dr. E. F. Castetter, University of New Mexico, Albuquerque, New Mexico, U. S. A.